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I. CONTINUOUS RAILWAY BREAKS.

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THE Report recently issued by the Royal Commission on Railway Accidents brings very prominently to notice the fact that the break-power at present ordinarily applied to passenger-trains is inefficient, and that to this fact is due the occurrence of many accidents which, by the application of better mechanical contrivances, might have been avoided. Former Committees and Commissions appointed by the Legislature to enquire into the subject of railway accidents have generally in their reports assumed that the principle of self-interest, in its influence on Railway Companies, should be relied on as the best safeguard against accidents; that the liability of the Companies to pay a serious amount in compensation in individual cases was a strong inducement to railway directors to work the line carefully, the Companies having thus a direct pecuniary interest in keeping their lines safe. These assumptions have not been borne out by subsequent experience. One of the members of the late Royal Commission, in a separate report, has shown to what extent the self-interest of Railway Companies has any practical bearing on the subject, and, in order to make the matter more clear, he has deduced from the expenditure returns made by Companies to the Board of Trade the following figures, which represent an average on the several classes of expenditure for a series of years:—

	Per cent.
1. Locomotive power...	30
2. Traffic expenses	28
3. Maintenance of way and works, &c.... ...	18
4. Miscellaneous charges, general, legal, and Parliamentary; rates and taxes, &c. ...	14
5. Repairs and renewals of carriages and waggons, &c.	9
6. Compensation for personal injuries, &c. ...	1
	100

From these statistics it will easily be gathered how far the Companies have a direct pecuniary interest in keeping their lines safe. The amount paid in compensation for accidents exceeds but very little a half per cent on their gross receipts: to this, of course, must be added something for injury to the lines and rolling stock caused by accidents, but no data are available for the calculation of the amount.

It thus appears that the pecuniary loss to Companies caused by accidents is almost infinitesimal, but the cost that must be incurred by them in order to provide many of the necessary means of safety represents by no means an inconsiderable outlay. But if the Companies refuse to face the matter boldly on public grounds, and in the interests of the travelling public, such alterations as may be necessary in the present control over the Companies must be given to the Board of Trade, or other Government department, by special legislative enactment. The Companies have been repeatedly warned, and they have as often neglected the admonitions addressed to them. The question now, therefore, rests with the public, and they will assuredly now speak out, in their own interests, in a manner that cannot fail to command respect from the most unimpressible Boards of Directors. The subject has been taken up by the scientific press, whence it will, in due course, be introduced to the public by the daily press; and the ball, having been once started, will certainly not cease to roll until the public have secured for themselves such immunity from accident on railways as can be afforded by the forced adoption of any known means of precaution or mechanical application.

From the Reports by former Commissions, and by the

Board of Trade Inspectors, it is manifest that a deficiency of break-power is one not of the least important causes at present responsible for accidents to passenger-trains. It is true that the speed of railway travelling has not increased sensibly within the last thirty years, but the conditions of train-service have materially altered within that period, and they are, by the effect of natural causes, continually changing in a manner which demands constant attention and the application of such means for facilitating traffic as may from time to time be devised for that purpose; for instance, the trains are more frequent and following each other more closely than before, block signal-stations are necessarily brought nearer together, whilst at the same time the weight of trains, consequent upon improved and more capacious carriages, and the increased number of passengers, necessarily attain a greater amount of *vis viva*, demanding the exercise of greater force in order to bring them to a stop even within the same distance as was formerly the case. The old-fashioned hand-screw breaks—which ought to have been abolished some years ago on all passenger-traffic lines of railway—are, viewed with the light of improvements which modern science has introduced, at best both clumsy and make-shift appliances for the purposes to which they are applied; and it was stated in evidence taken by the Royal Commission that the break-power at present generally applied is insufficient to stop a train within the distance generally existing between the distant signal and the home signal of a station. In the case of heavy trains, running at a speed of from 35 to 40 miles an hour, the ordinary screw breaks are insufficient to bring them to a stand under half a mile, and when travelling at a higher speed under 1100 or 1200 yards, whilst heavy fast express trains cannot—with the ordinary break-power—be pulled up in many cases under a mile and a quarter.

Besides the inefficiency of the power of ordinary screw breaks, another important objection to their use is the time required to bring them into action. The necessity for promptness of action in pulling up a train will be at once realised when it is remembered that in one second a train travelling at 60 miles an hour passes over 88 feet; at 45 miles an hour, over 66 feet; and at 30 miles an hour, over 44 feet. A train travels, that is to say, 100 yards—at 60 miles an hour, in 3·4 seconds; at 45 miles an hour, in 4·6 seconds; and at 30 miles an hour, in 6·8 seconds.

It has been stated, by one of the Board of Trade Inspectors, that if the continuous break system could be adopted it would be one of the most fruitful sources of saving collisions; and he further remarked that out of eighty-one accidents into which he enquired in one year, had continuous breaks been able to have been worked by the engine-drivers, at least in thirty-five cases the accident would have been mitigated, if not prevented altogether. The Royal Commission, in their recent Report, remark—"Accidents of the nature of collisions are generally the result of several contributory causes, but the amount of available break-power is obviously a matter of the greatest importance as a means of preventing them and of modifying their consequences. Our own enquiries confirmed the impressions which the Inspecting Officers' investigations of accidents led us to form, that not only was there generally an insufficiency of controlling power in trains, but also that the distance within which a train running at high speed could be stopped by the break-power ordinarily in use was not ascertained with any approach to accuracy." Consequently the Commissioners applied to the Railway Companies to institute a definite series of experiments, to test the amount of control given by the break-power ordinarily applied to their trains, and the effect of various systems of improved or continuous breaks. From the experiments carried out for this purpose it appeared that the amount of hand break-power usually supplied with the trains of the respective Companies failed to bring up the London and North-Western train within 2374 feet, that of the Caledonian Company within 3190 feet, that of the Midland within 3250 feet, that of the Great Northern within 3576 feet, and that of the Brighton within 3690 feet, the speed of the trains varying from 45½ to 48½ miles per hour. It must, however, be borne in mind that the trains with which these experiments were made were in the most complete order, and the guards and drivers had notice of the exact spot at which the signal to stop would be given. A large addition must therefore be made to those distances in practice, and unless much greater control is obtained over trains by additional break-power the Commissioners consider that to ensure safety the distant signals must be, for a level line, carried back to the distance of a mile. From the experiments made with continuous breaks, however, it is evident that there are ample means of controlling trains within much less distance by some of the various systems already in use.

Besides an improvement in break-power, it is also necessary that—whatever improvement upon the present system be adopted—a large proportion of the available break-power should be under the control of the driver, who is generally the first to become aware of apprehended danger. No matter how small the interval of time required for the driver to attract the attention of the guard, it may be of vital moment.

On this subject Capt. Tyler observed, in a paper recently read by him before the Society of Arts,—"When an accident occurs in which a carriage leaves the rails from failure in any portion of a train, it may be of great, and even vital, importance immediately to reduce the momentum of every part of it; and every extra second expended before this action is commenced may be a question of life and death." It will readily be understood how the safety of a train is increased by having the break-power under the control of the engine-driver as well as of the guard. In the case of a signal failing to work and to show sign of danger, the engine-driver will naturally, if an obstruction exists on the line, be the first to discover it; and, supposing the break not to be under his control, he must intimate danger to the guard by whistle, in the ordinary manner; but all this takes time, and between the interval of the driver's signal and the application of the break by the guard two or three seconds must inevitably elapse, during which interval the train has probably approached not less than 100 yards nearer to the impending danger, or nearer to fatal results. An instance in point has been given by Capt. Tyler, who, in the paper above referred to, cited an accident which occurred last November, near Wincanton, on the Somerset and Dorset Railway. In that case an up-passenger train for Bath was travelling at a speed of 35 miles an hour, when the leading wheels of the engine left the rails, from a defect in the permanent way. The engine ran thus for 200 yards before the driving wheels left the rails, but it then turned over on its side, 240 yards from the point of first disturbance. The engine-driver was killed, and the fireman and guard, who narrowly escaped with their lives, were severely injured. If, says Capt. Tyler, the engine-driver had been able at once to apply a continuous break throughout this train, on finding his leading wheels off the rails, it might have been pulled up with scarcely any damage to the rolling stock, and no injury to himself or any one else.

The same officer remarked, in his Report for 1872, referring to the great Railway Companies,—"It is mainly because sufficient attention has not been paid in past years to the various means of safety that the greatest Railway Companies of England appear so unfavourably at the head of the accident list." "The 238 train-accidents which occurred this year were all more or less of a preventable character. The means of prevention are well known, and have sufficiently often been urged, as well in individual as in general reports." Why these means of prevention have not hitherto been enforced upon the Companies is, that although it has been the practice of the Railway Department of the Board of Trade to urge upon Companies, by way of advice, the adoption of measures from time to time, as tending to diminish the risk of danger, they have no power of compelling the Companies to adopt them against the advice of their own officers.

In recent years no doubt many improvements have been introduced in the working of railways, in view to insuring increased security for passengers, all of which have necessarily been attended by increase of expenditure on the part of the Companies. So far as can at present be ascertained the prevailing weakness in our railway system just now is a want of efficient break-power; not that efficient breaks do not exist, but that the Railway Companies have hesitated too long to adopt them. The Royal Commission recommends that Railway Companies "should be required by Law to provide every train with sufficient break-power to stop it absolutely within 500 yards, at the highest speed upon which it travels, and upon any gradient on the line." This break-power, they further explain, should be sufficient to stop trains within 500 yards "under all circumstances," and Mr. Galt—one of the Commissioners—further explained, in a separate Report, that "the break-power that brings to a stand both portions of a train in case of its being divided by an accident is certainly the only kind thoroughly effective."

It is clear that nothing but a continuous break will satisfy the necessities of safety for railway travelling, as is shown by the evidences above referred to. There are many kinds of continuous breaks, however, and they have not all the same powers or properties, and in considering which is really the most efficient several considerations must be taken into account. On this subject Capt. Tyler has laid it down that a break should possess the following properties in order to render it thoroughly efficient, and safe under all but the most exceptional circumstances, against which, of course, no human ingenuity could devise adequate safeguards. A break, then, should be—

1. Simple and easy of control, especially by engine-drivers, but also by guards of trains.

2. Automatic in action in the case of an accident, or of a division of a train, without any interference from engine-driver or guards, so that the break-blocks may —on the separation of the couplings between any two carriages—be self-applied on every wheel.
3. Adapted to fly on, and not off, so that the blocks may be applied to and not released from the wheels, on failure of any of the parts, which would prevent the train from being started unless the couplings were complete and the whole apparatus in working order.
4. Instantaneous in its application—say to full force within one second of time—when operated by engine-driver or guard, or when self-applied.
5. Safe and simple in working—which does not necessarily imply simplicity in construction, as distinguished from risk and confusion in working.
6. Moderate in cost, as compared with efficient durability of parts and easy maintenance.
7. Capable of constant employment in the conduct of traffic, and not merely for employment in tests and in cases of emergency.
8. Provided with indicators for engine-drivers and guards, showing at a glance the condition of the break-power and the continuity of the connections.

The above conditions, coupled with those laid down by the Commissioners, that the break should be capable of bringing a train to a stand *under any circumstances* within a distance of 500 yards, seem sufficient for all practical purposes, and, if capable of being realised, not less than should be demanded in the interests of the safety of the travelling public. These conditions necessarily imply the use of a continuous break; but before considering which of the numerous inventions at present in use mostly fulfils them, it may be interesting to describe briefly the principles attached to each of the best of them respectively.

The several classes of continuous breaks at present in use may be divided according to the kind of power employed to actuate them. They may be classified as follows:—1. The chain break. 2. The hydraulic break. 3. The vacuum break. And 4. The air break. It is not pretended here to give descriptions of every kind of break that might be included under the above headings severally, but only of those which are unquestionably the representatives of each class, in consequence of their undoubted superiority, so far as has been hitherto ascertained by actual experience gained in the constant use of them in general work on lines of railway.

The following particulars relative to these breaks are taken from the Appendix to the Report of the Royal Commission, and may therefore be considered as describing those forms of each which are most perfect of their class:—

1. *The Chain Break*.—The latest improvement in this form of break is that known as Clark and Webb's continuous break. A chain—or, in the newest form, a steel rope—runs the entire length of the train, underneath the carriages, terminating in the guard's van, by means of which the breaks are applied to the wheels of every carriage. In the experiments made for the Royal Commissioners with this break, the train was divided into sections of four or five carriages, which were placed in the following order with reference to the break-vans:—First, there were four carriages together, then a break-van. From this van the breaks of the first four carriages were worked, and also those of the four carriages following the van. The remaining five carriages were under the control of the van at the tail of the train. Each carriage was provided with its own length of chain and couplings. The chain passed over seven pulleys fixed under the framings of the carriage, and under one pulley carried by levers which were in connection with tension-rods attached to the break-blocks. When the chain throughout a section was coupled up, one end of it was made fast to the end of the extreme carriage of the section, and the other was led to a chain barrel hung under the framing of the guard's van at the other end of the section, close to the centre axle of the van. On the chain barrel, and also on the van axle, there were friction-wheels, and by releasing a weighted lever in the van the friction-wheels were brought into contact, and, if the van was in motion, the chain barrel was made to rotate. By this means the chain throughout the section to which the barrel belonged was tightened, the pulley carried by levers under each carriage already mentioned was raised by the chain, and the breaks were applied to the wheels. The van from which the breaks of a section, both in front of it and behind it, were actuated, had two chain barrels set in motion by the release of one weighted lever. To release the breaks the guard in each van had to put back the lever into its normal position, and secure it with a catch, at the same time surrounding and releasing the breaks by a weighted lever arranged for the purpose. In order to place the continuous breaks of the whole train at the command of the driver, a cord was passed over the roof of the carriages to the engine. This enabled the driver to release the catch

holding the weighted lever in each guard's van, but the guards could only apply the breaks to their own section. There was, however, separate cord communication throughout the train. The breaks on the wheels of the vans themselves were applied by hand only. The guard could also, by pulling a signal cord attached to a whistle handle, call attention of the driver in case of danger.

However well this break may work under ordinary circumstances, it must be clear to anyone that it labours under several inconveniences, and could scarcely be relied upon in extremely exceptional circumstances. In the first place a chain is no stronger than its weakest link, and an imperfect weld or subsequent injury may remain undetected until the occurrence of an emergency, when the whole break-power of a train might be rendered useless by the breakage of a single link or the failure of a strand of wire-rope. Again, the means of placing the break-power within the control of the driver are complicated, and supplemental to the break-apparatus itself; besides which there is the difficulty of getting this break to act quickly, from the length of the buffer-strokes between the carriages, owing to which it was found, on the North London Railway, that more than 2 feet of chain had to be wound up for every carriage; and it was explained to the Commissioners that, where long buffers are used, in a train of eight carriages there would probably be sixteen revolutions of the carriage-wheels necessary before the slack chain was wound up sufficiently to put the breaks on to the wheels. This, with wheels 3 feet 6 inches in diameter, implies 168 feet run by the train, after setting the break-power in motion, before it begins to make itself felt in bringing up the train. In experiments on the Midland Railway accidents occurred through the break being applied too powerfully and too strongly, which caused the couplings to break and the train to part in two. The Carriage Superintendent of the Midland Railway further explained that, in working Clark's break it loses power every carriage away from the van, as the power gets less the further it is applied by the chain. Consequently the break bites tighter on some of the wheels than on others, which causes a slack or rebound. While some of the carriages are being very much retarded by the break, others are not so much retarded.

In pointing out the defects in this and other breaks which were brought to the notice of the Royal Commission, it must be understood that the object in view is not in any way to deprecate some and puff up other breaks, but to show in what respects each break requires improvements, so far as the evidence concerning them respectively given before the Royal Commission seems to indicate their weak points.

2. *The Hydraulic Break.*—Barker's hydraulic break, which is the representative of this class of break, was applied to the engine and the whole of the passenger carriages of an experimental train; but the tender and two vans were fitted with breaks worked by hand-power. The hydraulic apparatus, as used at the trials, consisted of the following parts:—On the engine there was a double-acting steam-accumulator, consisting of a large-sized cylinder with a piston in it connected with a plunger working in a second cylinder, which was kept filled with water from the tender-tank. The piston in the former was actuated by pressure direct from the boiler, without the use of any pump, and in making its stroke the plunger in the smaller cylinder was made to force water, with any degree of pressure, into pipes leading to small hydraulic rams attached to the engine and carriage breaks. These pipes led the whole length of the train, the connections between the carriages being made with india-rubber hose furnished with ordinary unions. Each carriage was fitted with two of the hydraulic cylinders and rams just mentioned, and these were connected directly to the break-blocks in such a manner that on the ram of each cylinder being moved by the pressure of water from the accumulator, the break-blocks on one side of the pair of wheels to which the ram belonged were forced against the wheels, while the pressure of water against the bottom of the same cylinder caused it to recoil, as it were, and draw the tension-rods of the break-blocks on the other side of the pair of wheels, thus clipping each wheel between its two blocks. In this arrangement, of course, the cylinders attached to the break-blocks have to be loosely suspended, and free to move horizontally for a distance nearly equal to the length of the arms. This description answers also for the action of the engine-breaks. By reverse action in the accumulator the pressure in the break cylinders can be relieved, and the blocks taken off from the wheels. The amount of pressure in the pipes can be regulated by a reducing valve; also, in order to keep the power always ready for immediate use, it is an essential part of this system that the pipes and cylinders throughout the train should be always full of water. The breaks were not arranged so that they would be self-acting in case of a train parting asunder.

The testimony given in favour of this break by those who had experience of its working was generally favourable; but it appears in the evidence that it is in no sense automatic, and it is entirely under the control of the guard, and not of the engine-driver. One necessity of its successful working is that the pipes should always be full of water, and in the event of a leak occurring in any part of the train its efficiency might be seriously interfered with, but when in proper working order it is no doubt a very powerful break.

3. *The Vacuum Break.*—Smith's vacuum break, which formed one of those experimented with by the Royal Commission, may be thus described:—On the side of the smoke-box of the locomotive there were two steam ejectors for exhausting air, operating conjointly, but acting independently in case one should part. Under each carriage and van throughout the train there was an india-rubber cylinder, of 15 inches diameter and 16 inches extreme stroke, stiffened with internal metal rings, and capable of collapsing and extending lengthwise. Under the tender there were two such cylinders. These cylinders were in communication with the ejectors on the engine by means of a double line of pipes, connected at the tail of the train and forming a complete circuit through it, with hose couplings between the carriages. On steam being admitted to the ejectors by the driver, the air is exhausted from the collapsing cylinders, and the movable end of each being connected with the ordinary break gear of each carriage, the breaks are at once applied. By opening an air-valve the cylinders refill, and the breaks are released. In addition to the above there was in the front and rear guards' van another arrangement for applying the breaks in case of emergency. This consisted of an air-exhauster in each van, nearly over one of the axles. On this axle a grooved friction-wheel was bolted and keyed, and in line with this another grooved friction-wheel of the same dimensions was suspended from the carriage in such a way that it could be thrown into gear with the first wheel or kept clear at pleasure. By means of a belt which passed up through the floor of the van, the second wheel, when set in motion, drove the wheel of a rotary pump-exhauster which was fixed in the van. The exhauster was made to run either way. Near the pump in the van there was a lever held up by a notch in a standard, and when the pump was required to work the lever was pushed out of the notch by a cam lever. By this operation the second friction-wheel under the carriage was thrown into gear with the axle-wheel, and, if the train was in motion, the pump was set to work and the air

exhausted from the collapsing cylinders under the carriages throughout the train. By means of a cord attached to the cam lever in the van, and running the whole length of the train and on to the engine, the driver, or any guard or passenger in the train, had the power of starting the exhauster. In addition to this there was a cord connected with the collapsing cylinders under the carriage, by which any movement in them was made to sound gong-bells in the guards' vans, and close to the driver on the engine. Also by the act of the driver exhausting the air from the collapsing cylinders, by means of the ejectors, the pump-exhausters in the vans are started.

The reports on the working of this break, where it has been practically tried, are generally very satisfactory, and there can be no doubt that it is a far more effective machine than either the chain or the hydraulic break. It however undoubtedly possesses two weak points; the one being the necessity for employing a cord communication to enable the driver to set in motion the break-apparatus in the guards' vans; and the other the employment of rubber sacks and rubber reservoirs, which, though somewhat cheaper in first cost than more durable materials, cannot be maintained as cheaply as iron cylinders and iron reservoirs, and, besides, they must be less reliable, and must at times be more apt to fail as they get old, when they are urgently wanted to act. The slightest cut or puncture would of course detract from their power, if it would not entirely neutralise their use; and this might easily be occasioned, in the event of a part of the train leaving the rails, by sharp stones thrown up from the ballast by the wheels, just when the efficient action of the break was of the greatest importance.

4. *The Air Break.*—The Westinghouse automatic air break is the best type of continuous air breaks brought before the Royal Commission. The mechanism of this break and the mode of operating with it may be thus described:—A small engine fixed on the locomotive, and deriving its steam direct from the boiler, worked a direct-acting pump, which forced air at pressure into a main reservoir, of nearly 9 cubic feet capacity, placed underneath the foot-plate. A line of tubing extended from the main reservoir longitudinally throughout the whole length of the train, with a cock at each end of each carriage. The connections between the carriages were formed of india-rubber hose and metal couplings. Under each vehicle a branch from the main pipe led through a triple valve—of special and complex construction—to a small supplementary air reservoir, and

also to two vertical cylinders provided with pistons fixed under the carriage and on either side of it, midway between the wheels. To the bottom of each cylinder was hung, by a pair of links, two cast-iron cams or quadrants. Each cylinder had a piston, the rod of which passed out through its upper end, and to the top end of this two links were attached. These links were of such form that they passed down the outside of the cylinder. Near their lower ends they were connected with the cams or quadrants just mentioned, and at their extreme lower ends they were connected with the thrust-rods of the cast-iron break-blocks. Thus, when the cylinder piston rises, it draws upwards with it the two links last mentioned, and in doing this the eccentric quadrants roll against each other, forcing the links apart, and, acting on the break-rods, thrust the break-blocks against the wheels. By the reverse of this action the breaks are made to leave the wheels. The breaks on the engine are similar in principle to the carriage-breaks, but the blocks were applied lower down on the wheels, and the arrangement of cylinders and links was somewhat different. There was one cylinder and set of break gear between the two coupled wheels on each side of the engine.

Upon a train being made up, compressed air may be allowed, by opening a three-way cock on the engine, to flow from the main reservoir and charge the whole of the main pipe and all the carriage reservoirs at a uniform pressure. When it is desired to apply the breaks, the compressed air is allowed to escape from the main pipe into the atmosphere through the three-way cock lately mentioned. The reduction of pressure to a small extent by this means operates upon a diaphragm in the triple valve under each carriage, instantly closing a port between the carriage reservoirs and the main reservoir, but permitting, at the same time, the air under pressure to pass from the reservoir to the break cylinders in proportion as the pressure in the main is reduced, thereby applying the breaks. By restoring pressure from the main reservoir to the main pipes the triple valves are shifted so as to charge again the carriage reservoirs, at the same time opening a discharge port in each triple valve by which the air can escape from the break cylinders, and thus release the breaks. The act of breaking asunder the train at any part would have the same effect as allowing the air to escape from the main pipe through the three-way cock on the engine, or through openings provided for the same purpose in the guards' vans or elsewhere.

It is impossible to read through the evidence given regarding this break before the Royal Commission without at once coming to the conclusion that in operation, both in this country and in America, its working has been all that could be desired. Besides possessing all the advantages of other continuous breaks, it can claim others which are not common to the latter. By no means an unimportant property of the Westinghouse automatic break is that its normal position is such as to apply the break-blocks to all the wheels of a train, so that the breaks have to be taken off by the driver before starting, and any defect in the mechanism would make itself at once known by the breaks refusing to be taken off in the ordinary manner. Thus infallible evidence is always given on the first starting of a train that the break gear is throughout in working order,— matter of no small importance so far as security in travelling is concerned. Another consequence of the automatic action of this break is, that if a train were broken into as many pieces as carriages, each carriage would be stopped by the self-acting application of the break upon its wheels. Also, the break can be applied by the guards as well as by the engine-driver, without the necessity of ropes or other extraneous appliances foreign to the mechanism of the break itself.

A description of the experiments conducted, with the several kinds of break in operation on different railways, by the Royal Commission, is given in Appendix F to their Report. From this some further very important particulars may be obtained relative to the comparative efficiency of the various continuous breaks at present in operation. Besides the general principles of their construction and application, their relative values must depend upon the results obtained from them in actual practice, and in calculating these several considerations must be kept in view; for instance, the speed with which the full force of the break can be applied; the mean retarding forces operating in each case; the distance within which a stop is effected; and the time necessary for taking off the break again—are all subjects of importance which must be taken into account in determining the relative values of different kinds of breaks.

In examining these several points it will not be necessary to review the results of experiments made with ordinary hand-breaks, as they are clearly so insufficient for the requirements of traffic at the present day that they may fairly be left out of consideration altogether. As regards the time occupied in the transmission of break-power through the trains, and in releasing breaks, the conditions under which the experiments were made did not admit of their being applied to Clarke and Webb's chain break. The observations made of several experiments with each of the other kinds of break gave the following average results :—

1. *Westinghouse Air Break.*—The time occupied in applying the break from the engine to the rear vehicle was from 1½ to 1¾ seconds, whilst the time occupied in taking it off was from 3 to 6 seconds.
2. *Smith's Vacuum Break.*—The time occupied in applying this break from the engine to the rear vehicle was from 4½ to 5 seconds, whilst to take it off required about 24¼ seconds.
3. *Barker's Hydraulic Break.*—With this break, if a coupling breaks, the rear portion of the train is placed beyond control of the continuous break, and therefore the report on its operations cannot be given as an average, but must be quoted in detail ;—

"First trial, from engine to fourteenth carriage, 5¼ seconds to put on, 8½ seconds to take off.

"Second trial, from engine to sixth carriage, 3 seconds to put on.

"Third trial, from engine to fifteenth carriage, not noted, 18½ seconds to take off.

"We subsequently severed the train between the eighth and ninth carriages after releasing the flexible pipe, leaving the valves open. When the engine moved on, a cord shut the valves, and the break could easily be applied by driver to the front portion of the train."

After giving particulars of the Westinghouse vacuum and Steele's air breaks, the Report proceeds :—"In these trials the most rapid action, both in putting on and taking off the breaks, was obtained from the Westinghouse air break."

The mean retarding force exercised by the several breaks was tried in a variety of ways, but it will not be necessary to note more than two methods which represented most nearly what would be the case in actual working. In both of these stoppage was ordered by flag signal, upon which all available break or other power was applied by driver and guards to the stopping of the complete train. In the first series of experiments sand was employed, whilst in the latter it was not used.

The following table shows the results obtained with the use of sand ;—

Train.	Break.	Mean Retarding Force, Percentage of Gross Load.
Midland..	Westinghouse Air ..	10·64 per cent.
London and North-Western	Clarke and Webb's ..	7·79 „
Midland..	Barker's Hydraulic ..	7·64 „
Great Northern	Smith's Vacuum ..	7·47 „

In the last three cases the total retarding forces were very nearly alike, ranging from 7·79 per cent to 7·47 per cent of the weights of the trains, or within 0·16 per cent either way of the mean result. On the other hand, the force acting on the Midland train with the Westinghouse air break ranged as high as 10·64 per cent, being 3·01 per cent above the mean result obtained by the other breaks. Without the use of sand the results were as follows :—

Train.	Break.	Mean Retarding Force, Percentage of Gross Load.
Midland..	Westinghouse	10·04 per cent.
„	Barker's Hydraulic ..	6·47 „
London and North Western	Clarke and Webb's ..	6·21 „
Great Northern	Smith's Vacuum ..	5·72 „

It is only necessary to remark here that the Westinghouse air break showed a percentage of retarding force 3·95 per cent above the mean of the results given by the other three breaks.

We have next to consider within what distance a train can be brought to a stand with the several kinds of break above referred to, and it will not unreasonably be concluded that the break which can be applied most expeditiously, and at the same time exercises the highest retarding force, will be found at the head of the list in this case also. The following table gives the distances (approximately) in which the stops would have been effected in each case under the influence of the same retarding forces referred to above, upon the application of break-power to trains running at the speeds of 30, 45, and 60 miles per hour :—

Train.	Break.	WITH SAND. Miles per Hour.			WITHOUT SAND. Miles per Hour.		
		30	45	60	30	45	60
		Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
Midland	Westinghouse ..	282	634	1128	300	675	1200
London and N.W.	Clarke and Webb's	385	866	1540	480	1080	1920
Midland	Barker's Hydraulic	393	884	1572	465	1046	1860
Great Northern ..	Smith's Vacuum ..	403	907	1612	525	1181	2100

Besides the above, a most important series of experiments was undertaken for the Royal Commission, in order to ascertain the effective power of the several breaks in the event of an accidental severance of a train. This was done by slipping a portion of the train where the break couplings led past; the point of severance being, in each case, fixed by the Commissioners at the time of making the experiment, so that it might not be known beforehand to the train attendants or patentees. In this way the severance could be taken to represent that which might occur through the accidental breaking of a coupling. The carriages were slipped from the train at full speed, and with full steam on the engine, on signal being given to the guard entrusted with the slip. The speed of the train at the time of the slip, the distance run in performing the stop by the severed portion, and the time occupied in the stop were carefully noted. Four trials were thus made; but the accidental breaking away of a portion of the London and North-Western train in one of the earlier trials afforded a fifth example, of more value, perhaps, than the rest, inasmuch as the breaking away was entirely unpremeditated. The hydraulic break, not being specially adapted to meet the contingency of breaking away, it was not tried in this series. The series produced the following results in reference to the slipped portions of the trains :—

Train.	Break.	Number of Vehicles Slipped.	Speed of Train when Slipped.	Distance Run in Stopping.	Mean Retarding Force, Percentage of Gross Load.
			Miles per hr.	Feet.	Per cent.
Midland	Westinghouse ..	12	51¾	869	10·13
London and N.W.	Clarke and Webb's	6	50⅓	928	9·27
Great Northern ..	Smith's Vacuum..	12	40½	2509	2·18

In the case of the Midland train the breaks were put on automatically over both sections of the entire train, with the exception of the engine and tender breaks, which were disconnected. The time of coupling up after the severed portions were brought together was noted: it took five seconds to couple the chains, and two and a half seconds to couple the hose-pipe of the breaks. In the case of the London and North-Western train a rather violent jar was produced when the coupling snapped. The severed portion consisted of five carriages and the rear van. The breaks of the said five carriages being actuated from the van, no collision took place between the two portions of the train, but, on the contrary, a space of 169 feet intervened between them after both had been brought to a stop. A special slip coupling with connecting-pipes and valves had been furnished in the case of the Great Northern train (Smith's vacuum break), but the Commissioners elected to effect the slip at another point, where no special provision for the contingency of breaking away existed, and where there was no valve in the hose-pipe. The action of the continuous break on the severed portion would doubtless have been more powerful had the influx of air into the severed pipe been prevented by a valve; but, as it was, the action was feeble compared with the others, as it amounted to less than one-fourth of that produced by either the Westinghouse air or by the Clarke and Webb's breaks.

These experiments having been conducted by wholly disinterested persons, and with the view of ascertaining really the most efficient break in existence, possess an especial value, and should unquestionably be relied on by Railway Companies as furnishing some guide to them as to which of the many breaks at present in existence should be adopted by them. No words that we might add could give additional force to the report on the experiments with the several breaks from which the foregoing particulars have been taken. From these it is clear that the Westinghouse air break is about 25 per cent superior to any of its competitors, and, from whatever point it is viewed, it is so pre-eminently more effective than the others that there should, we should think, be no hesitation on the part of the Railway Companies in adopting it. That it will be generally adopted by some of the leading companies there can scarcely be a doubt. Mr. Allport, of the Midland Railway Company, when under examination before the Royal Commission, expressed a very general approval of the Westinghouse break, and intimated that the Company was only waiting for the Commissioners' Report before adopting that break generally on their line; and it can scarcely be doubted that—except, perhaps, in cases where personal interest may bias the judgment of Directors, should such influence anywhere exist—the course adopted by the Midland Railway Company in this instance will be followed by other Railway Companies throughout the kingdom. It is most important that this should follow, for there is now so much interchange of traffic betwen railways, and the carriages of companies run through over several railway systems besides their own so generally, that some uniformity of system in the application of break-power should unquestionably be adopted. This, truly, is hardly a point for Government interference or for legislative enactment, but must be left to the good judgment and self-interest of the several Railway Companies, which ought to be sufficient to ensure the " survival of the fittest " amongst railway breaks. In conclusion, it may be remarked that if the conditions laid down by the Royal Commission be accepted as those which should be required to be fulfilled, there is only one at present in existence, viz, the Westinghouse automatic air break, which can be said to come up to the standard of efficiency laid down in those conditions.

Since the above was written a series of experiments were held at Cassel, from the 1st to the 4th of August last, the Report on which has just been published. Without entering into detail with regard to these experiments, it may suffice here to state that in each case the Westinghouse automatic break proved superior to the Heberlein, Steel, and Smith's breaks, with which it was in competition, to the extent varying in different cases from 16 to 60 per cent, thus completely corroborating the results obtained by the Royal Commission referred to above. With regard to the first cost of the breaks, the Westinghouse is slightly more expensive than either the Heberlein or Smith's, but it is far cheaper than the Steel break, whilst of the four the Westinghouse break is lighter in weight than any of the others by some hundredweights. It appears, however, from an official report on the subject to the Austrian Government, and from the report of the recent Belgian Commission, that the durability and small maintenance cost of the Westinghouse break compensate for its being somewhat dearer than others in the first instance.

In view of all these corroborating evidences it can be no source of surprise that the Westinghouse break has been adopted for the Belgian State Railways. It has, however, not yet been publicly notified, so far as we are aware, that the several Railway Companies in England have so far recognised their responsibilities towards the public as to fit up their several systems with the most efficacious break-power which has hitherto been introduced.

II. ON RESIDUAL PHENOMENA.

It is a well-known—indeed almost a trite—fact that the residues of manufacturing operations have proved a most fruitful source of novel and interesting bodies. If we wish to detect an element hitherto unknown we search for it among the refuse of metallurgical or chemical processes, in flue-dust, in furnace-soots, in slags, in burnt pyrites, in the mud of vitriol chambers, or in mother-liquors which have deposited their crop of crystals. If we are in quest of some new or valuable organic compound we operate upon the residue of the gas-works, in which Liebig prophetically declared that we might find whatever we wished if we would only seek intelligently; upon wood-tar, or upon natural products which have most probably undergone a process of destructive distillation in the recesses of the earth. In this manner we have obtained, on the one hand, bromine, iodine, selenium, cæsium, thallium, &c.; and on the other, phenol, rosanilin, artificial alizarin, and other the like marvels of modern chemistry. Nay, we sometimes elicit important products even from the residues of a residue. The "tailings" left from the manufacture of magenta are themselves a source of other colouring matters.

But we are not about to enlarge on the utilisation of waste products. That fecundity in novelties which seems to characterise refuse and residues is an apt type or illustration of the importance of "residual phenomena" as a source of unsuspected truths; and it is to such, as a sphere for discovery, that we wish to draw the attention of our readers.

What are "residual phenomena?" We will suppose a man of science setting to work to "verify" some theory—that is, to examine whether and in how far it accords with the facts it is intended to harmonise and to explain. There are here three cases possible:—In the first place, the theory in question may agree exactly and completely with the facts, leaving merely such minute errors as are plainly due to the shortcomings of experiment and observation. We may take, as an instance in point, the law of chemical combination in definite proportions. If we examine whether this is a correct statement of facts, we find it confirmed the more completely the more precise and accurate are our experiments. Secondly, the theory may distinctly fail to account for the phenomena before us, and may prove utterly contradictory to facts. Thus it was at one time maintained that the distinction between animals and vegetables lay in the presence of nitrogen in the former and its absence in the latter. But as soon as improved methods of analysis showed the presence of nitrogen in vegetable tissues the theory was recognised as at variance with facts, and was therefore abandoned. We may here briefly glance at certain phrases very common in the mouths of unscientific persons, of whatever stage of culture. They are apt, when encountered by an inconvenient fact, to tell us, in an off-hand manner, that there is "no rule without an exception," or sometimes even that "the exception proves the rule,"—a *dictum* quite on a par with Sir Thomas Browne's celebrated "*credo quia impossibile est.*" Now we should like any speaker or writer of this class to find us an exception to the law of universal gravitation, or to the doctrine of definite combination just mentioned. Let him produce, *e.g.*, an element which will combine with oxygen, or with sulphur, or with chlorine in every conceivable proportion, and yet form not mere mixtures, but a true compound or true compounds. Having found such an element, let him further show how by its existence the "rule" of combination in definite proportion is "proved," or, indeed, other than completely refuted. Still there is a sense in which this saying carries with it a certain amount of truth, and which perhaps explains its origin. An *apparent* exception, if it does not prove, may at least confirm a rule, so soon as its nature is understood. Thus we know that every kind of wood, if dry, is combustible. If some person asserted the existence of an exception to this rule, and brought forward in proof, *e.g.*, a piece of the fossil wood of Antigua,—*i.e.*, silica deposited in the exact texture of fragments of wood which have been long ago decomposed, it could of course be shown that this substance was wood in appearance only, and that its incombustibility was therefore no real exception to the rule. Or, to take another somewhat more complicated instance:—Everyone who has any acquaintance with chemical and physical science knows what is meant by the law of isomorphism; yet to this law, when first promulgated, there appeared a signal exception. Arsenic and phosphoric acids were at once recognised as compounds mutually analogous, and coming within the scope of the law; yet their soda-salts, as then known, did not exhibit that identity of crystalline form which the law of isomorphism requires. On closer investigation, however, it appeared that the ordinary phosphate of soda of commerce differs decidedly in its composition from the arseniate of soda, and, further, that there exists another phosphate of soda agreeing with the arseniate both in composition and in crystalline form. Such explanations of apparent exceptions may, perhaps, to some minds seem to "prove" or at least confirm the "rule," on the same principle that lawyers consider the title to an estate strengthened if it has been unsuccessfully called in question. Yet what truly confirms the rule is not the exception, real or apparent, but the detection of the fact that it is no exception at all. Before anyone can venture to pronounce the imagined "law" that all hybrids are necessarily barren to be "proved" by instances to the contrary, he should have prepared himself to show that such instances are contrary in seeming only, and not in reality.

We are thus brought to the threshold of the third possible case:—The theory or the law may agree with or account for the facts to a certain extent, leaving, however, a margin unexplained. Such margins are the "residual phenomena" which we are about to consider: their value as a clue to further discovery will best appear from example. We may begin with one of the simplest cases, which, nevertheless, led to the discovery of the alkaline metal lithium. One of the foremost consequences of the chemical law of combination in definite proportions is that all samples of any compound, supposing them pure, must contain the same ingredients in the same relative quantities. A specimen of sulphate of magnesia, whether natural or artificial, whether prepared recently or a century ago, contains exactly the same proportions of magnesia and of sulphuric acid. On a certain occasion the Swedish chemist Arfwedson, having been engaged with the analysis of a mineral petalite, obtained what he at first took to be sulphate of magnesia; but on closer examination he was led to doubt this conclusion, not by any striking discrepancy in colour, solubility, or taste, but by an excess of weight. His suspicions being thus aroused, he soon found that he had in his hands an element as yet unknown.

Turning from chemistry to physics, we find other examples equally striking and simple. Thus the speed with which sound travels through the air had been deduced with great precision, from its known cause and mode of propagation. But when the conclusions thus reached were brought to the test of actual experiment, the agreement, though approximate, was not complete. The result was sufficiently close to show that the calculation had been, in the main, based upon correct principles, but there was still a margin, a "residuum" of velocity unaccounted for. Whilst examining into the discrepancy, Laplace suggested that the heat liberated by the compression of the air resulting from sound-vibrations might be the missing factor. Its influence was accordingly calculated, and was found to supply the complete solution of the difficulty. Nor was this all; the law of the evolution of heat on the compression of bodies received at the same time a most signal verification.

These few instances will suggest certain reflections. Residual phenomena, though possibly of very frequent occurrence, will be overlooked save by the patient and accurate investigator who is content with nothing less than certainty. Had Arfwedson quietly assumed that the saline body formed during his operations was sulphate of magnesia, without subjecting it to any quantitative examination, lithia would probably have remained undiscovered for some time, and the honour would doubtless have fallen to the share of some other chemist. Had the comparison between the experimental and the theoretical velocity of sound been made in a careless manner, the discrepancy would have escaped attention, and the question ultimately solved by Laplace might never have been raised. But there is another danger to which Liebig has drawn attention, and which he has happily illustrated by an incident in his own early career. The experimentalist may perceive a difference between theory and practice, between results or properties actually observed and those which in his opinion were to have been expected, but may content himself with framing some hypothesis to account for the difficulty without resorting to any actual investigation. Such a line of conduct prevented Liebig from anticipating Balard in the discovery of bromine. He had actually obtained the new element in a state of approximate purity, but had assumed it to be chloride of iodine, had imagined an hypothesis to explain its peculiarities, and had set it aside unexamined. This event, as he tells us, served him as a caution for the rest of his career. How many residual phenomena, which have been really noticed, escape investigation in virtue of some superficial and baseless explanation we can scarcely even conjecture.

But another question arises here:—How is a residual phenomenon to be distinguished, on the one hand, from those errors of observation and experiment which cannot be absolutely avoided, even by the most careful operators, and, on the other, from those discrepancies which prove that a law or a theory is essentially erroneous, and must be rejected. It is scarcely possible, we fear, to lay down any absolute rule which shall in all cases direct the enquirer how to overcome this difficulty. Certain suggestions, may, however, be given. In case of a theory or a law which is correct, and which fully embraces and accounts for all the facts, the discrepancies will be very minute; they will be smaller in proportion as the experimentalist is more skilful, and they will further decrease as suitable precautions are taken and sources of error are eliminated. In the case of a false theory, on the other hand, the errors will be much larger, will vary in different instances, and, instead of being reduced by improved methods and more finished manipulation, are more likely to become greater. Residuals differ from both these cases: the correspondence between the fact and the theory cannot to a certain extent be denied, but there is a margin which, though generally small, is constant, and cannot be lessened by any niceties and refinements of procedure. Where the law or the theory under investigation is merely qualitative, more must be left to the tact and judgment of the observer.

In carefully searching for the source of an anomaly we may discover someting other and much greater than we originally intended or hoped. The old apologue of the farmer's sons—who by digging for an unknown treasure increased the fertility of their land, and reaped a harvest of unimagined luxuriance—finds its frequent realisation in the history of scientific research.

We shall, perhaps, find it instructive to turn from the consideration of residual phenomena which have been recognised as such, and which have been fully traced to their causes, and examine certain laws which admittedly do not fully agree with the phenomena, and whose shortcomings demand explanation. Such laws may, of course, belong to our second class, and in that case require total rejection; but they may possibly be not so much erroneous as incomplete, embracing a part only of the phenomena, and leaving the rest still unaccounted for.

Considerable attention is now drawn to the relations existing between the atomic weights of our received elementary or simple bodies. If these bodies have been evolved from a common source, as a variety of considerations conspire to suggest, it is highly probable that such relations should exist. If, on the other hand, they are primordially distinct, and, as far as we can perceive, accidental in their number, in their distribution, their relative amounts, and their properties,—suppositions highly repugnant to our intellect,—then no laws of numerical relation between their atomic weights may be traceable.

The earliest attempt in this direction was made by Prout.

Perceiving that the atomic weight of hydrogen was lower than that of any other element, he put forward the law that bears his name,—*i.e.*, that if we take the atomic weight of hydrogen = 1, the atomic weights of all the other elements will be multiples of 1 by some whole number. The suggestion agreed tolerably well with the state of chemical knowledge in his day. Not a few atomic weights, calculated on the hydrogen standard, had been found to be whole numbers, and there was at least room to anticipate that, on further and more accurate research, the fractions with which the atomic numbers of the remainder were encumbered might prove to be the result of error. There was, moreover, about the proposed law an appearance of "simplicity" which could not fail to recommend it to general notice. Let us hasten to declare that we put little faith in such simplicity. It is a conception totally *ex parte hominis*. Wheresoever we have attempted to interrogate Nature somewhat closely, we have utterly failed to perceive that simplicity, as it appears to man, is any part of her plan. When, therefore, a law or a theory is recommended on the ground of its simplicity our suspicions are aroused, and we fear lest the facts have been garbled and manipulated.

On the other hand, a certain amount of confirmation is lent to Dr. Prout's view by the observations of astro-spectroscopists, that hydrogen predominates in the stars whose temperature seems highest, and where the dissociation-process must be most active. Hence it is contended hydrogen may be a more primitive stage of development of the primordial element, or elements, than any other substance with which we are acquainted. Still, admitting such to be the nature of hydrogen, the truth of Prout's law is by no means a necessary consequence. The more accurate determination of the atomic weights of the elements, again, has by no means, as was anticipated, tended to free them from fractions. Hence the value generally attached to Prout's law has not latterly been increasing. Dumas, who is one of its most distinguished upholders, proposes the modification that all atomic weights are multiples by a whole number, if not of 1, yet of 0·25 or 0·50, maintaining that out of fifty-eight atomic weights not more than half-a-dozen differ appreciably from multiples by whole numbers of half the atomic weight of hydrogen, whilst some of the exceptional elementary weights are multiples of one-fourth the atomic weight of hydrogen. Here, however, we enter upon dangerous ground. If, multiplying 1·0 by a series of whole numbers, we obtain products coinciding with the atomic weights of the elements, the coincidence is of considerable value, and may be fairly interpreted as indicating a profound relation between hydrogen and the other elements. But if we multiply 0·5 or 0·25 by the same numbers, and especially if we reserve to ourselves the right of using either of these factors if the other do not suit our purpose, the coincidences will naturally become more frequent and less significant. By taking a sufficiently minute fraction of the atomic weight of hydrogen (say 0·001), and multiplying it by a series of arbitrarily-selected whole numbers, we cannot fail to obtain the atomic weights of the elements, whether any natural connection exists between them or not.

In opposition to Dumas, Prof. Stas infers—from his own justly-famed researches on the atomic weights of nitrogen, chlorine, sulphur, potassium, sodium, lead, and silver—"that there exists no common divisor between the weights of simple bodies which unite with each other to form definite compounds." He considers, therefore, the hypothesis of Prout as altogether illusory, and regards the reputed elementary bodies "as distinct entities, having no simple relation of weight one to another." This conclusion he founds upon his own recent determinations of the weights of certain elements,—a train of research executed with such precaution, skill, and patience that it may serve as a model for all similar investigations.

Thus if we take hydrogen = 1, we shall have—

Oxygen	15·960
Silver	107·600
Nitrogen	14·009
Bromine	79·750
Chlorine	35·368
Iodine	126·533
Lithium	7·004
Potassium	39·040
Sodium	22·980

These numbers are, of course, utterly hostile to the pretensions of Prout's hypothesis to completeness. Not a single exact multiple of 1 by a whole number do we find. Nor are the deviations from whole numbers so small as to be neglected. Least of all can it be assumed that the fractions which occur are the result of any inaccuracy, and may consequently be obliterated by further and more minutely-accurate research. Those who read the detailed account of the experiments of Professor Stas, and note the precautions he has taken to eliminate all sources of error, will be unable to conceive of any further improvements adequate to convert the fractions here given into whole numbers.

But let us re-consider the atomic weights of the nine elements above quoted. In three cases only do we find the fraction extending to the first decimal place; in four others it commences with the second decimal, being always less than 0·05; whilst in the remaining two elements the difference between the atomic weight experimentally determined and the calculated figure is confined to the third decimal. If we suppose, with Dumas, that the common divisor may be 0·5, we have the following differences between the experimental and the hypothetical nnmbers:—

Oxygen	0·040
Silver	0·100
Nitrogen	0·009
Bromine	0·250
Chlorine	0·132
Iodine	0·033
Lithium	0·004
Potassium	0·040
Sodium	0·020

Or, as Prof. Stas himself expresses the matter, "The greater part of carefully-determined weights come so near to calculated ciphers that it has been necessary to have recourse to all the arts and refinements of analysis to prove that they are not absolutely exact." But if we take the weights as given by earlier authorities, and, it may be admitted, determined, if not with less skill, yet without the aid of the most recent appliances, we find the difference between the experimental result and the calculated number in many cases even greater than those shown by the researches of Prof. Stas.

Thus the older numbers are—

Oxygen	16·0
Silver	108·1
Nitrogen	14·0
Bromine	78·4
Chlorine	35·4
Iodine	126·0
Lithium	6·4
Potassium	39·2
Sodium	23·2

The difference between these figures and a whole number, or the fraction 0·5, is therefore—

Oxygen	0·0
Silver	0·1
Nitrogen	0·0
Bromine	0·1
Chlorine	0·1
Iodine	0·0
Lithium	0·1
Potassium	0·2
Sodium	0·2

Or an average of 0·0888, whilst according to the atomic weights as determined by Stas, the margin is only 0·0688. The divergence, according to the older atomic weights, reached the first decimal place in six out of the nine instances, whilst in the numbers of Stas we find this occur in three cases only. Thus far, therefore, the labours of the illustrious Belgian chemist are more favourable to Prout's hypothesis than might appear at first glance. An eminent authority also calls attention to the fact that "the smallest atomic weights, which, as a general rule, are those of the best known and most easily estimated elements, accord the most precisely with Prout's law." Thus in addition to the instances of oxygen, nitrogen, lithium, and sodium, we have carbon, whose atomic weight has been re-determined by Dumas and Stas as 6·0 and by Liebig as 6·008, and sulphur, which Stas has investigated along with the above-mentioned nine elements, and which—taking oxygen as 15·960—he finds to be 31·976, showing a divergence from the calculated cipher of 0·024. According to the old notation, oxygen being—

$$\frac{15{\cdot}960}{2} = 7{\cdot}980,$$

sulphur would be—

$$\frac{31{\cdot}976}{2} = 15{\cdot}983,$$

the difference from the whole number being then merely 0·017.

If, then, we take these eleven elements,—viz., oxygen, silver, nitrogen, bromine, chlorine, iodine, lithium, potassium, sodium, carbon, and sulphur, and fix our attention on the first decimal place, we shall find it scarcely what might have been expected if we had to deal with a set of distinct entities between whose atomic weights there was no definite relation. According to the doctrine of probabilities, there would be no reason why we should expect any of the ten digits to preponderate over another; yet in this instance the decimal place in question is occupied nine times out of the eleven by figures which are more in favour of the hypothesis than against it, whilst in two only—the cases of bromine and chlorine—do we find figures which hold an unequivocally half-way position between the whole number and the fraction 0·5. This coincidence we can scarcely attribute to accident. Some reason there must be why the fractions should be either a little in excess of the whole number, a little below it, or else bear a similar relation to 0·5.

If, then, this state of facts is utterly adverse to our reception of Prout's hypothesis as a complete and accurate law, it is, we submit no less incompatible with the antagonistic view of its being utterly baseless and imaginary. The case is apparently one of a "residual." Prout's hypothesis is interfered with and modified by an x, which prevents the resulting atomic weights from being either exactly whole numbers or merely fractions midway between two such whole numbers. What is this unknown law no one has yet, we believe, made any systematic attempt to discover?

The error, if we may so call it, is as we perceive by no means constant; sometimes it is in excess, and sometimes in deficiency. It appears also to vary with the class of the elementary bodies. It is considerable in the Halogens, chlorine showing a deficiency of 0·132, bromine a deficiency of 0·250, and iodine an excess of 0·33. These numerical peculiarities are not easy to account for, especially in the case of bromine. We might have expected that it would have shown a smaller deficiency, as compared with the calculated cipher, than does chlorine, forming thus a transition to the excess perceived in the atomic weight of iodine. It is remarkable that bromine shows in another manner this tendency to deficiency. Its atomic weight, as determined by M. Stas, is 79·750; but as calculated from the weights of its congeners, chlorine and iodine, it is—

$$\frac{35{\cdot}368+126{\cdot}533}{2}=80{\cdot}950,$$

thus showing in the actual weight a deficiency of 1·200 to be accounted for.

The group of the alkaline metals, lithium, sodium, and potassium, is less markedly exceptional. Here, as the atomic weights rise, we have in lithium an excess of 0·004, in sodium a deficiency of 0·020, and in potassium again an excess of 0·040. The atomic weight of sodium, calculated from those of lithium and potassium,—

$$\frac{39{\cdot}040+7{\cdot}004}{2}=23{\cdot}022.$$

is in excess of its experimental weight only by 0·042.

The atomic weights of the alkaline earthy metals are unfavourable to the hypothesis of Prout, or at least point to the more decided interference of some unknown cause.

According to Marignac the atomic weight of calcium is 40·21, and that of strontium 87·25,—numbers of a very intractable character, and which, lying in an intermediate position between a whole number and the fraction $x{\cdot}5$, would seem as favourable to the total rejection of Prout's law as to the prospect of its exceptions being explained as residual phenomena.

Lead, as determined by Professor Stas, has for its atomic weight 103·136 (o = 15·960), or an excess of 0·136. Thallium, according to the careful determination of Mr. Crookes, = 203·642. Here, then, the excess is also 0·142, very closely approximating to the unexplained margin in the case of lead.

Until, however, the atomic weights of all the elements shall have been verified with the precautions and refinements employed by M. Stas in the case of silver, nitrogen, the halogens, and the alkaline metals, and by Mr. Crookes in the case of thallium,—a task which no single chemist can possibly complete in the longest life-time,—all speculations concerning the relations of their combining weights must be regarded as premature. It may be that when all the required numbers are before us the deviations in excess or deficiency from what Prout's hypothesis would in strictness require will be found to display a "periodic" character. All that can be said at present amounts to very little more than the recommendation to suspend judgment till the facts of the case are fully before us—a consummation, we fear, scarcely to be hoped for in the life-time of the present generation. Seventeen years ago the late Sir John Herschel, in an inaugural address as Chairman of the Chemical Section of the British Association, remarked—"Not until these numbers are determined with a precision approaching that of the elements of the planetary orbits—a precision which can leave no possible question of a tenth or a hundredth per cent, and in the presence of which such errors as are at present regarded as tolerable in the atomic numbers of even the best determined elements shall be considered utterly inadmissible—I think can this question be settled ; and when such gigantic consequences—so entire a system of Nature—are to be based on a principle, nothing short of such evidence ought, I think, to be held conclusive, however seductive the theory may appear."

The so-called atomic heats of the elementary bodies bring us in contact with another instance of residual phenomena. It has been observed that within certain limits the atomic weights of the simple bodies vary inversely as their specific heats, and that consequently the products of these two numbers, or the so-called atomic heats, are approximately a constant quantity. In very many cases the numbers thus found range from between 5·9, as in aluminium and rhodium, to 6·9, as in iodine. This is certainly a rather wide deviation from the requirements of the law. But there are several sources of uncertainty which may be supposed to affect the results of the calculation. The atomic weights themselves, as we have just had occasion to remember, are not yet ascertained with absolute certainty. The purity of the various specimens of the elementary bodies operated upon is in some cases open to doubt. The determinations of the specific heats themselves may not have been entirely free from experimental error, and the circumstances under which these determinations were made cannot be pronounced strictly comparable. The experiments were performed within temperatures nearly the same in an absolute point of view, but not relatively identical—*i.e.*, not equidistant from the fusion-points of the bodies concerned. The difficulty of obtaining the elements in comparable conditions is no little increased by the fact that not the temperature of a substance alone, but its physical structure and its state of aggregation, have a modifying action upon its specific heat.

But there are three elements, mutually analogous in many respects, which show a much wider discrepancy from the law than can be explained by such considerations. The atomic heat for boron in the graphitic state is 2·59, in the crystalline condition 2·75. Carbon, as wood charcoal, gives the number 2·90, as graphite 2·41, and as diamond 1·76. These figures, moreover, widely as they differ from what the law might require, are obtained by taking the atomic weight of carbon = 12. Silicon when crystallised shows the atomic heat 4·97, and when fused 4·90. It is obvious that these three elements cannot be made to harmonise with the presumed law by any known expedient. Even if we were to fix the atomic weight of carbon at 24, the results, 4·82 for graphite and 3·92 for diamond, would still remain distinctly anomalous. The only method of saving the law is the assumption that we have here before us a residual phenomenon, some other law exercising a modifying result, either on these three elements alone or at least to a greater extent than upon other simple bodies. It must be remembered that the atomic weights of the three elementary bodies concerned are low, and that of carbon, at least, may be regarded as most satisfactorily ascertained, so that the solution of the difficulty cannot lie in that direction.

It is a somewhat curious circumstance that though the law of Dulong and Petit, as it is called, has been now for a long time before the scientific world, and though the anomalies to which we have drawn attention have been recognised for more than a quarter of a century, the explanation is still wanting. Kopp, as the result of his investigations, concludes that the law does not hold good for all the commonly reputed elements in their solid state; but if so we naturally inquire into the reason of its limited scope, and without some reply we feel inclined to doubt whether its approximate applicability in other cases may be anything more than a casual coincidence. In the meantime the law of Dulong and Petit should be regarded as on its trial, and the weight allowed it should be limited indeed. It would, for instance, be scarcely justifiable to reject any new hypothesis because it should happen to lead to results incompatible with the law in question.

In the instances we have been considering, and in all others of an analogous nature, there can be no doubt whether, or in how far, the law is in harmony with the phenomena which it embraces. But where the results are incapable of being stated numerically a serious difficulty arises. Issue may be joined on the fundamental question whether the theory agrees with the facts or not, and a thoroughly indisputable decision is not easily arrived at. Hence the recognition of residual phenomena—not to speak of their explanation—is far less easy in biology or geology than in astronomy, physics, or chemistry. One and the same theory may be by different authorities accepted as satisfactory and complete, rejected as altogether illusory, and again admitted as a partial view of the truth, which, however, leaves many points still in the dark. To us it seems that the origin of species supplies an admirable instance of residual phenomena. The laws of "natural selection" and "sexual selection," collectively known as Darwinism, do, as far as we can judge, account satisfactorily for a large portion of the phenomena concerned; but as, undoubtedly, there is another portion which they cannot by any amount of ingenuity be made to explain. For instance, it has been well said that before natural selection—or indeed selection of any kind—can be brought into play, variation must have already set in. This will be at once apparent on the following consideration:—Suppose a pair of animals, or, still further to simplify the matter, a single hermaphrodite, being of low type, existing in the primæval world, had produced a hundred fertile ova. Two cases then are only possible: the young animals springing from these ova must be either all absolutely alike, or they must exhibit certain variations, however slight. In the former alternative there is no basis for natural selection to work upon, the very idea of selection implying differences among the objects among which a choice is to be made. In the latter case, the varieties, being *ex hypothesi* antecedent to the action of natural selection, cannot be its effects. Here, then, we have a residual phenomenon, a marginal fact for which the Darwinian hypothesis is unable to account, and which will yet have to be explained before we can understand the origin of species. Nor in the instance we have supposed can the explanation be furnished by Lamarck's hypothesis. The influence of all external circumstances acting upon the parent animal must affect the ova, if at all, equally; but, as regards the first beginning of variation among a number of creatures absolutely alike at birth, the Lamarckian view has the advantage over the doctrine of natural selection. If the young animals wander away from the place of their birth they may become exposed to different influences, from climate or from the quantity and quality of their food, and among their progeny slight variations may thus possibly appear, and thus enable the principle of natural selection to come into play. Successive broods produced by one and the same parent may also be supposed to differ from each other to a small extent if any change has occurred in the circumstances to which the parent is exposed. But it is difficult to avoid the conclusion that the "residual" here is an internal tendency to vary, faint instances of whose action we see in the facts that no two children of the same parent, no two symmetrical organs of the same animal, no two leaves even of the same tree, are absolutely alike.

If we carefully trace the instances in which both facts are imperfectly ascertained, and laws accepted or rejected without full verification, we shall feel that a new *Skepsis Scientifica* is urgently demanded in the present day.

III. THE ACTION OF LIGHT UPON THE COLOURATION OF THE ORGANIC WORLD.

UNTIL the earlier portion of the present century light, by the vast majority of civilised persons, was regarded as a medium for the sense of sight, and as very little more. With the discovery of its chemical functions, brought home to the popular mind by the invention of photography, a revolution in opinion took place, and the danger now is, not that its real powers should be overlooked, but that it should be credited with effects in which its part is very doubtful. It has been especially proclaimed to be at once the creator and the destroyer of colouration in the organic world. The superior intensity of the light to which they are exposed has been pronounced the chief cause why diurnal species are more gaily coloured than their nearest nocturnal allies, and why the flora and the fauna—especially the insects and the birds—of tropical regions are so rich in hues of a gorgeous character. It may therefore be not uninteresting to inquire into this supposed double function of light, and ascertain, if possible, its limits in either direction. In so doing it will be impossible for us to overlook the views put forward by Mr. A. R. Wallace in a recent issue of "Macmillan's Magazine."

The bleaching power of the sun's rays, and to a less extent of ordinary diffused daylight, has been fully recognised in the affairs of daily life. It has been observed that this same agency, utilised formerly in preparing vegetable fibre for the reception of colours, gradually destroys, in almost every instance, the work of the dyer and the printer, and exerts a corresponding influence upon the hues of plants. There is, however, a distinction by which its effects upon the integuments of animals are limited.

It is well known that what we designate as colour may be produced either by the interference or by the absorption of rays of light, and hence the colours of animals may be divided into two well-marked classes. On the one hand, especially in birds and insects, we find hues which are iridescent changeable according to the relative positions of the observer and of the light, and are possessed of an intense, so-called, metallic lustre. Such colours—to take familiar examples—may be seen in the plumage of the peacock, of the starling, on the wings of the "purple emperor" butterfly (*Apatura Iris*), on the entire coating of the rose-beetle (*Cetonia aurata*), of the fire-wasp (*Chryseis ignita*), and of many other common native insects. In the vegetable kingdom they may be pronounced unknown. Such colours are due to the interference of certain rays of light, whether reflected from superimposed transparent films or reflected from or refracted through minute striæ. These colours are permanent, even on the most prolonged exposure to air, to atmospheric moisture, or to full sunlight. Unless the very texture of the feather, the wing-scale, the elytron, &c., be destroyed by putrefaction or combustion, the colour remains unhurt. Nor can we by any means extract from such coloured surfaces a dye or pigment capable of being applied to other objects.

On the other hand, there are colours which do not change their shade from whatever position they are regarded, and which possess little of that intense lustre which marks the former class. To this kind belong the colours of all flowers, of caterpillars, of the great majority of our native butterflies and moths, and, in short, of the vast bulk of organic beings. These colours are due to the absorption of certain of the rays of light, such absorption being effected by substances known as pigments, and capable, when present in sufficient quantity, of being extracted by solvents, and used to dye or stain other bodies. Such colours have not the permanence of the first-mentioned class. Every entomologist knows that if a case of butterflies be kept constantly exposed to the sun, or even to diffused daylight, then—no matter how completely air, damp, and mites may be excluded—the specimens fade, even though the minute scales which clothe the wings may still be found in their places. Yet the golden spots on the wings of the *Plusiæ* and the pearl-mother markings of the "fritillaries" remain unchanged. The colours of most other insects behave in a very similar manner. Beetles are generally supposed to wear a more permanent livery; but every Coleopterist must have observed how the reds of ladybirds, of *Aphodius fimetarius*, of *Elater sanguineus*, &c., lose their purity and brightness on exposure, and to some extent even on preservation in darkness. Even darker and more intense colours are gradually affected. Thus in the collection of native beetles in the British Museum, which have doubtless been exposed to the light for some years, the jet-black *Typhœus vulgaris*—absurdly known as the "bull-comber"—has taken a decided chestnut-brown, whilst a similar change has come over the blue-black elytra of the common dung-beetle.

To test the speed of the bleaching power of light upon deep-coloured Coleoptera we placed in a glass case, outside a south-western window, specimens of the following species:—*Cetonia aurata*, *Eupœcilia Australasiæ*, *Typhœus vulgaris*, *Geotrupes stercorarius*, *Abax striola*, and *Sternocera orientalis*,—and exposed them to the sun during the months of June, July, and August, 1876. The *Cetonia* and the *Sternocera*, whose colours are of the interference-class, were unaffected; but the black of the *Typhœus*, the *Geotrupes*, and the *Abax* was changed to a brown, and the brown of the *Eupœcilia* to a very dirty yellow. Thus we see that even the darkest and most intense pigment- or absorption-colours are affected by light. This fact accounts for one class of the variations in colour met with in different specimens of one and the same species. An insect that has lived long and has been much exposed to the sun may have more degraded colours than such as are captured soon after reaching full perfection.

If we examine the nature of the changes produced by the action of light we shall notice the following facts:—Pigment greens, blues, lilacs, pinks, and roses—shades not very abundant in the animal kingdom—are the first to fade. Full reds, purples, and blacks resist longer. Oranges, yellows, fawns, drabs, browns, and olives have still greater permanence, merely taking a duller or dirtier tone. The changes ensue in a definite direction. Blues and pale greens turn to a grey or a yellowish drab; darker greens to an olive; lilacs, pinks, and roses to various shades of grey; reds become a reddish or yellowish brown; purples a very dirty brown; yellows and oranges verge more to a pale brown, and may rank as buffs or fawns. The alteration is therefore from the primary or secondary towards the tertiary colours, accompanied with a decrease in depth. But we have never seen a primary colour, when fading under the influence of light, pass into another primary colour; nor does any secondary or tertiary colour ever pass into a primary. The change which blacks undergo will not seem surprising if we reflect that in Nature, as well as in Art, they generally consist of an intense olive or brown to which a deep blue or purple is superadded. The latter hues, being the more fugitive, fade first on exposure to light, and thus a dirty olive or a rusty brown must remain.

These changes are in partial harmony with what we observe in the vegetable kingdom. A dull, dirty brown is the ultimate goal towards which leaves, flowers, and fruits, as well as insects, tend while fading; but those splendid intermediate changes which we find in autumnal foliage have nothing analogous in the decaying colours of insects.

It is curious that in the manufacture of those artificial colours which now play so important a part in tinctorial operations a corresponding rule holds good. If these dyes, during their elaboration, are submitted to a heat too high or too prolonged, the product becomes dusky, and a dirty brownish grey is the final result.

We must further note how, in the animal and vegetable kingdoms, pure and bright colours are connected with the highest vitality only. We plant the dusky seed in the earth amidst the dark remains of decomposing organic matter, and as it grows up we see it put on higher and higher colours, till, in the culminating moment of its life, in the act of inflorescence, prismatic hues are all but universal. Then begins the process of decay, attended by a degradation of colour. Similar changes may be traced in animals. Externally we need merely compare the dull-coloured larva with the brilliant imago, or the sombre-coated nestling with the brighter plumage of the mature bird. Internally we may contrast the intensely-vitalised scarlet arterial blood with the darker-coloured and more contaminated venous blood, and still further with excrementitious matters. The great truth to which we are here calling attention has not altogether escaped the notice of Mr. Wallace, who writes—"The very frequent superiority of the male bird or insect in brightness or intensity of colour, even when the general tints and colouration are the same, now seems to me to be due to the greater vigour and activity and the higher vitality of the male. The colours of an animal usually fade during disease or weakness, while robust health and vigour add to their intensity.* This intensity of colouration is most manifest in the male during the breeding season, when the vitality is at a maximum." But we are not aware that either Mr. Wallace or any one else has fully grasped the principle laid down above, or traced its numerous applications, æsthetic as well as biological.

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* Those who are brought practically in contact with animals have long been familiar with the fact that a "dull coat" is indicative of disease, or at least of weakness.

But among the "pigment-colours" there is a very great diversity in permanence due to the nature of the colours themselves, or to that of the tissues in which they inhere. Dr. Hagen divides such colours into epidermal, placed in hair, in feathers, and in the chitinic exo-skeleton of insects, and hypodermal, situate in the softer internal layers of the skin. That the latter are the more easily affected by any external influence is natural.

Alterations and degradations of colour similar to those above-mentioned may indeed, under certain circumstances, be produced even in the absence of light. But we have direct experimental evidence to show that, other things being equal, animal matters retain their colours most completely in the absence of light, and fade the more rapidly in proportion to the intensity of the illumination to which they are exposed. Hence we are compelled to recognise light as a destroyer of animal colouration.

But light is generally regarded not merely as a colour-destroyer, but as a colour-producer, and it is with this its supposed function that we have now to deal. Those who take here the affirmative view rely mainly on two facts, or supposed facts, to which we have already briefly referred,—the higher colouration and the superior brilliance of the tropical fauna, and the sombre hues of nocturnal and subterranean beings. At these facts we must look, and seek to ascertain their meaning. We must of course admit that Europe produces no humming-birds or trogons, no *Belionotæ* or *Pachyrhynchi*; but we must also remember that the total number of species of birds, of reptiles, and of insects found, say in South America, is far greater than the sum total existing in Britain or on the European continent. Hence, even if the tendency to produce a gay colouration were equal in either case, the probability is that South America would be the richer in gorgeous species. Again, travellers who visit tropical countries not unnaturally select the most showy forms, and their collections are therefore not a fair average. Naturalists, such as Mr. Wallace, who have taken the trouble to examine closely, find that even in New Guinea, Borneo, or Brazil dull-looking species exist in numbers. Had we catalogues of the insects of such countries as complete as those we possess for Britain, France, or Germany, our views as to the general character of a tropical fauna would be doubtless modified. As the insects of warm climates, also, are upon the whole larger than those of our hyperborean latitudes, they necessarily attract attention, and their beauty does not pass unseen; yet every entomologist knows that even in Britain we possess "tiny miracles of Nature" which, if viewed with a lens of low power, display a splendour little—if at all—inferior to the most richly attired tropical species. We will merely mention, as instances, *Chryseis ignita*, *Chrysomela cerealis*, *Donacia proteus*, *Polydrusus micans* and *flavipes*, *Rhynchites betulæ* and *populi*, *Lampra rutilans*, and *Anthraxia salicis*. *Calosoma sycophanta*, also, if very rare in Britain, is very common in certain parts of Central Europe, and may be fairly considered one of the most gorgeous species of the entire family of Carabidæ to be met with in any part of the world.

The case, then, seems to stand thus:—We have in Britain certain species, small, and it may be rare, which display the very same shades of colour and the same brilliance as we find in the most admired forms of tropical life. This fact seems to us scarcely consistent with the theory that the more intense light of low latitudes is a prominent factor in the production of splendid colours. Were such the case gaily-coloured species in our climate would not merely be fewer and smaller; they would rather be altogether wanting.

Again, different portions of the torrid zone differ very widely as regards the number, and even the beauty, of the richly-attired birds and insects they produce. Thus, as Mr. Wallace has pointed out, in New Guinea 50 per cent of the birds are brilliantly coloured, whilst in the Malay Islands and in the Valley of the Amazon the proportion does not exceed 33 per cent. Can this distinction be rationally ascribed to any excess of light enjoyed by New Guinea over and above the amount received by the Valley of the Amazon? Both these respective districts lie under the Equator; both are fruitful, plentifully supplied with moisture, well-wooded, and exposed—as far as we can perceive—to very similar meteorological conditions. But if excess of light cannot be the cause of the superiority of New Guinea over equinoctial Brazil, why should it be put forward to explain the superiority of Brazil as compared with Britain? Why should the fauna of the Philippine Islands, as is remarked by Mr. Wallace in his invaluable "Glasgow Address," be so rich in species of exceptionally splendid colours? Can there be in those islands either any excess in the quantity or any peculiarity in the quality of the sunlight? That there is, no one has yet even attempted to show, and were such the case it would doubtless be traceable in a variety of phenomena not limited to the organic world.

Another important point has been raised by Mr. Bates. He shows that whilst in many tropical butterflies the males are most splendidly coloured, the females—in numbers of cases at least—are sombre and insignificant in appearance, so much so that in former times they were often regarded as specifically distinct from their mates. If excess of light, therefore, be the producing cause of the splendour of the tropical Lepidoptera, why should not the effect appear alike in both sexes? To this argument, however, the reply has been made that in these very species the females are exceedingly sedentary in their habits, remaining generally concealed in shady thickets, whilst the males flutter about in the sunshine, and, being thus more exposed to light, experience modifications which—transmitted with constant accumulation from one generation to another—have produced the splendour now characteristic of their sex. To this question of the relative amount of exposure to light in different stages of existence we shall have to return.

But the amount—or at least the intensity and clearness—of the sun does not necessarily vary with latitude alone. The air of some countries is more transparent, less obscured by fogs and clouds than that of others. More light evidently reaches the earth's surface on open plains or on table-lands and in deserts than in dense forests and in narrow valleys Do we find any corresponding variation in the prevalent hues of the animal population of these respective localities? Mr. Wallace points out that the most brilliantly-clad birds and insects are dwellers in the forests where the amount of light received is comparatively scanty. On the other hand, in the deserts, where—as we have already mentioned—light must attain its terrestrial maximum, the prevalent colouration, if not dark, is certainly neither light nor brilliant. As the Rev. H. Tristram remarks, in such regions the smaller Mammalia, the birds, the snakes, and lizards are alike sand-coloured, their hues having evidently more reference to concealment than to the influence of an intense illumination. There is indeed, if we wish to come to details, a curious want of harmony in the effects which light is expected to produce. We know that it bleaches in certain cases and darkens in others; but it is no easy task for us to predict when either of these opposite effects will be manifested. Still it is perfectly possible that light might have a bleaching power upon some living organisms, and a darkening effect upon others, according to their different molecular structure. There is, for instance, little doubt but that the air of Persia is, as a rule, exceedingly transparent; the climate is dry, mists and clouds comparatively rare, woodlands scanty, and the country generally open. We have even heard it stated that there the satellites of Jupiter are occasionally visible with the naked eye. Here, therefore, we have doubtless a case of light in its greatest intensity; but, according to Mr. Blanford, Persian specimens are generally paler than their nearest European representatives. Here, if light be directly concerned, its action must be of a bleaching character; yet we generally find in mammals, in birds and reptiles, as well as in insects, the upper surface, or portion most exposed to the sun, is darker than the under side, or than parts generally kept in the shade. An animal in whom the contrary arrangement prevails—such as the common badger—has much of the appearance of a caricature. This darkening of the superior surface of animals is again adduced as an instance of the chromogenic power of light, a view to which we shall afterwards take occasion to revert.

As regards the comparison between the tropical and the extra-tropical faunæ the case may, perhaps, be fairly summed up thus:—There are certain cosmopolitan groups whose members, wherever found, are alike devoid of rich or brilliant colouration; there are other groups—such as the Ornithoptera, the Papiliones, the Buprestidæ, the Cetoniadæ, the trogons, humming-birds, birds of paradise, &c.—which have a remarkable and hitherto-unexplained tendency to the development of splendid hues, and which, if not exclusively tropical, have their head-quarters and produce their largest representatives within the torrid zone. Other groups, again, attain their greatest splendour beyond the tropics, as, *e.g.*, the ducks, the pheasants, and among insects the ground-beetles or Carabidæ. It has, indeed, been suggested that if the colder regions of the earth are now inferior to the tropical districts in the beauty of their fauna, the cause may be sought in the ravages of the Glacial epoch. If the most magnificent species were forest-dwellers, as we now find it to be the case in warm climates, their destruction would be almost inevitably involved in the desolation of their haunts and the annihilation of their food. Perhaps, too, the very splendour of such supposed forms would render them more conspicuous to their enemies, and thus accelerate their extirpation. All such speculations, however, are little more than conjectural. We conclude indeed, judging from the fossil remains of insects discovered at Œningen and elsewhere (See "Quarterly Journal of Science," vii., 255), that certain groups, now mainly tropical or subtropical, were very extensively developed in Central Europe; but at the same time we find indications that the climate, at least as far as warmth is concerned, was almost tropical in its character.

We may next inquire whether the relative brilliance of colour in various animal groups is at all connected with their diurnal or nocturnal habits, or with their greater or less exposure to light at different stages of their development. It is a truism that the diurnal Lepidoptera are upon the average much more highly coloured than the nocturnal species, the moths. Some weight has been laid on the circumstance that in butterflies both sides of the wings are freely exposed to light, and that both are also adorned with a variety of hues, whilst in moths, where the under surface of the wings is not turned to the light, it generally exhibits a dull and uniform colouration; but these facts admit of much qualification. Even among the small number of beetles indigenous in Britain there are some—such as *Erebia Cassiope*, *Cœnonympha Davus*, and *Thanaos Tages*—certainly less brightly coloured than many moths. Many species of butterflies, also, if richly coloured on the upper surface of the wings, can boast no gay or varied tints beneath. We need only mention the common peacock (*Vanessa Io*). Again, in certain genera of moths we find colours as vivid as can be met with in butterflies—*e.g.*, *Callimorpha*, *Euchelia*, *Chelonia*, and *Catocala*. The most remarkable feature in these genera is that the chief display of colour appears on the upper surface of the hind wings—a part as little exposed to light as the lower surface, since when the insect is at rest, in the daytime, it is completely screened by the anterior pair of wings.

In the larva state it cannot be said that Lepidopterous insects are much exposed to light. As a rule the caterpillars of the diurnal as well as of the nocturnal species prefer shade to sunshine. It is perhaps somewhat curious that the habits of the larva stand in no regular connection with the diurnal or nocturnal character of the mature insect.

Turning to the Coleoptera, we find further facts unfavourable to the supposed predominant influence of light upon the development of colour. Such Coleopterous larvæ—and they are the majority—as live in total darkness are, indeed, generally of a dull dirty grey, contrasting strongly with caterpillars which are more or less exposed to light, and many of which exhibit a bright and pleasing colouration.

This circumstance, like the etiolation of plants reared up in darkness, is certainly in favour of the view that light is not without influence upon organic colouration; but, on the other hand, let us consider the after-life of some of these dull-looking beetle-grubs. The most gorgeous, perhaps, of all Coleoptera are the Buprestidæ. These creatures spend the whole of their larval and pupal life within the trunks of trees, and consequently in total darkness. When mature, indeed, they sport for a time in the chequered sunlight of the woodlands. But why, if light be the main cause of animal colouration, should they be so far superior in brilliance to the Longicornes, or wood-beetles, which from birth to death are exposed to precisely the same circumstances? Taking the opposite extreme, the Staphylinidæ—of which the common "devil's coach-horse" is a familiar example, rank in appearance among the dullest and least decorated of all the insect tribes, whether they inhabit cold or warm climates; yet these creatures, instead of leading the earlier part of their life in complete and constant darkness, are active when larvæ, and may be seen running about in the daylight, seeking for prey. Surely, therefore, being so much more exposed to light than the Buprestidæ or the Cetoniadæ, they ought, on the theory we are examining, to be at least correspondingly beautiful. Let us turn to the Melolonthidæ, of which the common and destructive insect known as the cockchafer may serve as the type. Their early life is spent in darkness, since when larvæ they live underground, devouring the roots of plants. When mature their colours must be pronounced far less brilliant than those of their near allies, the rose-beetles (Cetoniadæ), which are equally nursed in darkness. It will be of course objected that the adult cockchafer is a nocturnal—or at least a twilight-loving—insect, while the rose-beetle feeds and flies by day. We will therefore take another instance—that of the Elateridæ, or click-beetles. As larvæ they, like the immature cockchafer, live underground, but when mature they are diurnal in their habits; yet the general colouration of the family is what some people call "sober," scarcely more gay than that of the Melolonthidæ, and forming a most striking contrast to that of the Buprestidæ, whom they so closely approach at once in their structure and in the degree of light which they encounter, both in their earlier stages and in mature life. Again, we may consider the weevils (Curculionidæ), all of them when larvæ burrowing from daylight in the interior of fruits and in the buds and stems of plants; yet when mature some of them—*e.g.*, the diamond-beetle—are as remarkably brilliant as others are conspicuously sombre.

On the other hand, attention is drawn to the Chrysomelidæ, to which the redoubtable Colorado beetle—vilely called the potato-bug—belongs, a family very richly and brightly coloured. Their larvæ are active, and they are thus throughout their lives exposed to the sunshine.

Among the animal population of the seas and rivers, also, we meet with facts, not a few, difficult to reconcile with the hypothesis under examination. It must be admitted that in all waters, save the very shallowest, the amount of light enjoyed must be very decidedly less than that which falls upon the surface of the land in similar climates; yet we do not find that the denizens of the waters are, as a general rule, less vividly coloured than those of the dry land. On the contrary, fishes, crustaceans, molluscs, besides aquatic forms lower in the scale of existence, such as the sea-anemones, display all the colours of the rainbow in a purity and in a profusion rivalling what we observe in the most gorgeous birds and insects. We admit that splendid oceanic forms are more abundant in tropical waters than in higher latitudes, and also that in a majority of cases the inmates of shallow waters are more vividly coloured than the dwellers in deeper, and consequently darker seas. But what must be inferred from the following observations, extracted from a paper by H. N. Mosely, late Naturalist to the *Challenger* Expedition, read before the Linnean Society on February 15th, 1877?—"A species of *Edwardsia* from 600 fathoms has undergone but little modification from the littoral form. The *Cerianthus* from 2750 fathoms is like its shore-brethren. Thus one species is found in shallow water at the Philippines, under the full glare of the tropical sun, while another species exists at 3 miles depth, where solar rays never penetrate, and where the water is at freezing-point. The deep sea-anemones retain vivid colours in the dark."

This fact is very suggestive. It agrees ill with the often-expressed view of teleologically-disposed naturalists, that all the brilliant hues of animal and vegetable life have been called into existence for man's delectation; but no less does it clash with the conclusions drawn from the paleness and obscurity of certain nocturnal, subterranean, or cave-haunting animals, such as the Coleopterous larvæ to which we have referred, wood-lice, crickets, &c. Light, it would seem, is not the sole condition for the production of positive colour; nor are the dwellers in darkness necessarily restricted to a garb of whites, blacks, and greys. It can, further, scarcely be contended that the land-shells of any country are more vividly and intense coloured than the marine shells of its coasts, many of which are as highly decorated within as without; yet a land-shell will doubtless receive a larger share of the solar radiations than a sea-shell.

Again, whilst there is thus abundant proof that an aquatic or even a deep-sea existence is not necessarily incompatible with a rich colouration, we find certain groups—the aquatic insects—ordinarily plain in their hues. The water-beetles, chiefly frequenting shallow pools and rivers, present ordinarily a dark olive, black, or brown colouration, relieved at most with rusty yellow, and those of tropical climates show little, if any, pre-eminence in this respect over their allies from colder regions. But these beetles, be it noted, if devoid of splendour, are not etiolated. The water-scorpion, water-boatman, and other aquatic Hemiptera, though living rather on than in the water, and fully exposed to light, are also remarkably plain in their colouration.

We have repeatedly referred to nocturnal animals; but it will be observed that in the higher forms of life the common views concerning their dominant colours scarcely hold good. Thus the owls, though not decked out with any metallic hues, differ little in the general character of their colouration from their diurnal kindred, the hawks, presenting bold, well-defined patterns, and a variety of black, fawn, brown, buff, and white shades. Few mammals display more vivid hues than the Felidæ, most of which are unquestionably nocturnal. Many nightly or subterranean insects, also, such as *Sphodrus leucopthalmus* and *Pristonychus terricola*, show no signs of etiolation. Even the common cockroach makes no approach to that pallid, ghastly hue which is commonly supposed characteristic of animals inhabiting sunless localities. Amongst nocturnal species we believe few, if any, instances can be found where the male surpasses the female in brightness or depth of colouring.

Mr. Wallace, however, whilst going perhaps even farther than we should be prepared to accompany him in the rejection of the theory which regards animal colouration as directly proportionate to the intensity of solar radiation, gives some curious instances of phenomena proving that in certain cases light has a direct action upon the colours of organic beings. Thus Mr. T. W. Wood, some time ago, pointed out that the chrysalids of the small "cabbage white" (*Pontia rapæ*) varied in colour when the larvæ had been fed up in boxes lined with different coloured materials.

Those which were kept in black boxes were nearly black, whilst such as had lived in white boxes were almost white. He observed corresponding changes in the same species in a state of Nature; chrysalids fixed against a whitewashed wall being whitish; those secured to a red brick wall being reddish; whilst those fixed against a pitched paling were nearly black. The cocoon of the emperor moth is also observed to be either white or brown, in accordance with the colours of surrounding objects. A still more decisive instance of such changes has been observed in the chrysalis of *Papilio Nireus*, a South-African butterfly which has been studied by Mrs. Barber. It acquires, more or less exactly, the colour of any contiguous object. "A number of the caterpillars were placed in a case with a glass cover, one side of the case being formed by a red brick wall, the other sides being of yellowish wood. They were fed on orange-leaves, and a branch of the bottle-brush tree (*Banksia*) was also placed in the case. When fully fed some attached themselves to the orange twigs, others to the bottle-brush branch—and these all changed to green pupæ; but each corresponded exactly in tint to the leaves around it, the one being a dark and the other a pale faded green. Another attached itself to the wood, and the pupa became of the same yellowish colour; while one fixed itself just where the wood and brick joined, and became one side red, the other side yellow."

This Mr. Wallace pronounces "a kind of natural photography, the particular coloured rays to which the fresh pupa is exposed in its soft semi-transparent condition effecting such a chemical change in the organic juices as to produce the same tint in the hardened skin." This power of the pupa to assume the colour of closely adjacent objects, however, is limited, since when Mrs. Barber surrounded one of her caterpillars with a piece of scarlet cloth the pupa displayed its ordinary green tint, though the small red spots with which it is marked were rendered abnormally bright. It must be recorded, however, that these very interesting changes are confined to the chrysalis, and do not appear to have extended in any way to the mature butterfly. We have never been able to trace any modification in the colours of butterflies reared, for one generation, in abnormally-coloured light, nor, as far as we are aware, has any other observer been more successful.

A correspondence has also been in some instances traced between the colours of animals and those of the localities which they inhabit and the food which they eat. Spiders have been found of exactly the same tint as the flowers in which they lurk. Mr. Wallace, on the authority of Sir Charles Dilke, mentions a pink-coloured *Mantis* which, when at rest, closely resembles the pink flower of an orchis, and is thus enabled to seize unsuspecting butterflies. But we should be wrong in ascribing such similarity of colouration to the effects of reflected light, or, indeed, of any merely physical influence. They are almost certainly due to physiological causes, and are instances of what is called "protective colouration."

There is another class of phenomena which at first sight seems due to the action of light. Many insects when they first emerge from the pupa are abnormally pale, and do not take their full mature colouration until after a longer or shorter interval of time. It was in virtue of this property that an entomologist, commissioned by the German Government to inspect a field where the dreaded Colorado beetle had made its appearance, was enabled to decide that these insect enemies had only just appeared in the mature form, and that on turning up the ground a further stock would be found in a rudimentary state, as on actual trial was found to be the case. But this gradual development of colour has not been proved to be the result of light. We have reared up caterpillars in perfect darkness, and have found their colours on reaching maturity no less brilliant than those of their fellows which had been exposed to light in the ordinary course of nature. In the case of interference-colours a change in the physical condition of the integuments, consequent of their drying and hardening on exposure to the air, is doubtless necessary for their development. The evolution of the pigment-colours we are at present investigating, and believe that it is simply due to a process of oxidation.

Some other of the phenomena advanced in support of the "light theory" of organic colouration may also be, with great probability, referred to other causes. Thus some ascribe to light the fact that the upper surface of most animals is more intensely coloured than their under side. Many fishes have a dark back, and a pale greyish blue or greenish belly; but, as Mr. Wallace points out, this arrangement seems more protective than due to the action of light. An enemy—say a sea-bird—looking down from above will have difficulty in distinguishing the dark back of the fish amidst the water. On the other hand, an enemy looking from below will see the pale belly of the fish against the dull bluish colour of the sky as seen on looking up through the water, and will scarcely detect its presence. Now, were this arrangement of colours reversed the fish would be much more readily seen, either from above or from below, and the chances of its escaping from its enemies would be much reduced. At the same time it must be confessed that this explanation is not admissible in all cases of a similar arrangement of colour. Thus in many Crustaceans unable to swim, and therefore not liable to be seen by any enemy from below, the under surface is much paler than the back. Similarly slugs—whether creeping upon the ground, upon the stems or leaves of vegetables—are liable to be espied from the back or the sides, but never from beneath; yet in most cases their under surface is decidedly paler than their back. These instances, and others which might be adduced, certainly seem to agree better with the supposition of a darkening influence due to the freer action of light upon the upper side than with that of a protective distribution of colour.

But from the whole of the evidence before us, especial attention being paid to the case of the deep-sea anemones, we are forced to conclude that the colouration of an animal species is not, in the mathematical use of the word, a function of the amount of solar radiations to which it is exposed. That this conclusion does not compel us to deny the influence of sunlight upon the hues of all animals, under all conceivable circumstances, scarcely needs to be stated.

The fact that Lepidopterous larvæ are in a majority of cases, partially at least, of a green colour is not inexplicable. They retain in their bodies, in an undecomposed state, the chlorophyll of the leaves upon which they have fed. Larvæ, on the other hand, whether Lepidopterous or Coleopterous, which feed not upon leaves, but upon wood, roots, seeds, &c., not containing chlorophyll, may naturally be found deficient in this green colour, without our taking the presence or absence of light into account. Hence we need not wonder that the caterpillars of the goat-moth and the wood-leopard, the larvæ of the Longicornes, Buprestidæ, Dynastidæ, &c., are not green: they have not been consuming a green pigment. But why have we comparatively so few green butterflies and moths, and so many green birds and green beetles? The green colours found in birds and in beetles—with the exception of such forms as *Cassida*, a *leaf-feeder*, are due not to a pigment, but to the interference of light, so that their formation must be explained on different principles. The paucity of green butterflies may, perhaps, be traced to the fact that chlorophyll is a mixture of two colouring principles,*—cyanophyll, which is blue, and xanthophyll, which is yellow,—the latter of these colours being much more permanent than the other. Hence if, as appears exceedingly probable, chlorophyll is assimilated by leaf-eating insects, a number of phenomena connected with their colouration become at once intelligible. We have mentioned in an earlier part of this paper that among animal tints pigment-greens are generally the first to fade, and that they become a dull yellow or a yellowish drab, as may be observed in a specimen, say, of *Cassida equestris*, which, however carefully preserved, soon loses its pale green hue, and turns yellowish. The reason of this change, we contend, is that the cyanophyll or blue colouring-matter first undergoes decomposition, while the yellow xanthophyll alone remains. A similar change, taking place in the living animal in its pupa condition, is the cause why pigment-greens are so rare alike among Lepidoptera and Coleoptera, whilst yellows and browns of different shades are so exceedingly common, and relatively so permanent. We find also that certain caterpillars, green in the earlier part of their life generally, though not invariably, take a brown colour as they approach the time of their assuming the pupa state.

But even supposing that chlorophyll is demonstrably assimilated or deposited in the tissues of certain insects, the hypothesis we have been suggesting takes us but a very little way. We have still to ask why the green colour in certain species remains undecomposed to the mature condition, whilst in others it disappears in the pupal or even in the larval state, and how, after disappearance or absence in the larva, as in *Chœrocampa Elpenor*, it appears in the perfect insect? We have to enquire why certain diurnal caterpillars, consuming as much chlorophyll as do any others,—*e.g.*, *Vanessa Io*, *V. xanthomelas*, *V. urticæ*, &c.,—are free from a green colouration? At the same time we must admit that in caterpillars of this class a yellow pattern is very rarely absent, as if the xanthophyll had already been separated from the cyanophyll. We have to explain the pigment-blues, of which there seem to be two, if not three, the identity of which with cyanophyll must not be too rashly assumed, though in many cases we see both blue and yellow spots appearing in a butterfly, as if the two colours, which in its earlier state had been blended together, were now separated, as in *Papilio Machaon.* We have, further, to throw a light upon the origin of the pigment-reds, to two of which Mr. Wallace refers as being different in their chemical constitution and behaviour.

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* Some authorities consider that chlorophyll is a mixture not of two, but of three colouring principles (Fremy and Cloëz), or of four (Stokes). As these, however, are in all cases found to be respectively blue and yellow, the view we have taken will not be affected by these discordant results.

But chlorophyll is not the only substance which has been called into requisition in order to explain the mysteries of animal colouration. It has not escaped the attention of biologists that all those creatures which develope, more or less frequently, beautiful hues are precisely the same in which uric acid is abundantly secreted,—*i.e.*, birds, reptiles, insects,—whilst in the Mammalia, in which the secretion of uric acid is trifling, the prevailing colours are dull. It was asserted that whilst uric acid is abundantly found in the excretions of parrots, humming-birds, &c., at other times of the year, during and immediately before the moulting season it was absent. Hence the inference that this compound might play a part in the elaboration of the new plumage was not unwarrantable. In addition came the fact that a beautiful violet colour, known as murexide, and capable of producing a variety of shades, was artificially obtained from uric acid.* Unfortunately when these investigations were carried on the distinction between interference-colours and absorption-colours had not been fully apprehended, and the iridescent hues of humming-birds, trogons, *Belionotæ*, were supposed to be due to some peculiar pigments of unknown composition. Nor has it, as far as we are aware, ever been shown that the excreta of splendidly-coloured birds are richer in uric acid than those of sea-fowl. For the present, therefore, the uric acid theory must be considered as useless.

A consideration of the food of different species might at first sight appear likely to throw some light upon the nature of their colouration. But we find intense splendour and varied tints alike among carnivorous species (Cicindelidæ and certain Carabidæ), wood-eaters (Buprestidæ), and leaf-eaters (Chrysomelidæ). We find dull and sordid colours among many carrion- and dung-feeders (Silphidæ, Aphodiidæ, Staphylinidæ), whilst others addicted to a similar diet—such as most species of the genus *Phanæus*—display the most splendid hues. Nor is an examination of the diet of birds more satisfactory.†

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* Murexide, known in the commercial world as "Roman purple" and "Tyrian purple," was some time ago prepared from guano,—*i.e.*, the excreta of sea-fowl,—and was in considerable demand among dyers and calico-printers. Being costlier than the coal-tar colours, it is now superseded.

† In addition to the case of chlorophyll above mentioned there seem to be individual instances where the colouring matter of a plant, if eaten by insects, may be traced in their secretions. We do not know whether the deep reddish

It may perhaps be thought that in an inquiry into the influence of light upon the colouration of animals a consideration of their diet, or of their peculiar secretions and excretions, is out of place. But whether solar radiations, or local atmospheric influences, or the need of protection take a greater or smaller share in the development of colour, there must be essential differences in the material upon which these causes act. Mammals are exposed to the same climatic influences as birds and insects, and are likewise exposed to dangers which they might escape by a colouration favourable to concealment. Their hair is, chemically considered, a material no less suitable for the display of gay and brilliant colours than are the feathers of birds, the scales of serpents, or the chitinous coating of insects; yet neither in lustre, in varying play of colour, nor in delicacy and elaborateness of design, do they make even the faintest approach to a rivalry with these groups of animals. There must therefore be an internal source of colouration, not everywhere present, upon which external influences may react.

Mr. Wallace, whilst rejecting the light-theory, brings forward certain principles which he considers throw a light upon the phenomena of colour in organic nature. Whilst demurring to the common conclusion that tropical light and heat are the cause of colour, he fully recognises the general fact that "all the more intense and gorgeous tints are manifested by the animal life of the tropics, whilst in some groups, such as butterflies and birds, there is a marked preponderance of highly-coloured species." This phenomenon he ascribes to a variety of causes, some of which yet remain to be discovered. The foremost place is given to the following consideration:—"The luxuriant vegetation of the tropics throughout the entire year affords so much concealment that colour may there be safely developed to a much greater extent than in climates where the trees are bare in winter, during which season the struggle for existence is most severe, and even the slightest disadvantage may prove fatal." Fully admitting the force of this consideration in the case of birds, we must yet, with all the deference due to so eminent an authority as Mr. Wallace, point out that it can have very little moment as regards insects which during the winter are in a dormant condition, as larvæ or

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violet liquid which exudes from *Timarcha lævigata*, an insect feeding upon bedstraw, a plant of the madder tribe, has ever been examined for alizarin or purpurin.

pupæ, either in the earth, in the trunks of trees, or other localities where neither beauty can betray them nor its lack screen them from the pursuit of any enemy.

As the first among the causes of colouration he places the need of protection. He points out that browns and other tertiary colours, being most readily produced by " an irregular mixture of many kinds of solar rays, are most likely to occur when the need of protection is slight, or even when it does not exist at all, always supposing that bright colours are not in any way useful to the species." Hence browns, olives, and other dirty colours may naturally be expected to predominate.

Brilliant colours, again, often serve as a sign that their wearer possesses some unpleasant or dangerous property, and hence warn possible enemies to pass on and seek some less nauseous prey. The number of apparently feeble and defenceless species which are clad in the most conspicuous colours, and which are avoided and refused by birds, monkeys, spiders, &c., is astonishing. The present writer, in a paper read before the Entomological Society (Trans. Ent. Soc., 1877, Part III., p. 205) has shown that, in a great number of cases at least, the most showy and conspicuous caterpillars feed upon plants either absolutely poisonous or possessing offensive flavours and odours, whence the rejection of such larvæ by insectivorous animals. Their brilliant colouration is therefore simply a danger-signal.

The theory of " Sexual Selection," upon which Mr. Darwin lays great weight, Mr. Wallace finds himself unable to accept as in any way an explanation of the distribution of colour in animals. He remarks that " whilst male butterflies rival, or even excel, the most gorgeous male birds in bright colours and elegant patterns, there is literally not one particle of evidence that the female is influenced by colour, or even that she has any power of choice, whilst there is much direct evidence to the contrary." In the case of the silk-moth Mr. Darwin admits that " the females appear not to evince the least choice in regard to their partners." On the principle of natural selection among a number of rival male butterflies, "the most vigorous and energetic" will probably be successful, and, as these properties are very generally correlated with intensity of colour, natural selection " becomes a preserver and intensifier of colour." Very similar is the case among birds. We know that in many species the male displays his colours and ornaments, but, as Mr. Wallace contends, there is a tota absence of any evidence that the females admire, or even notice, this display. "The hen, the turkey, and the pea-fowl go on feeding while the male is displaying his finery) and there is reason to believe that it is his persistency and energy, rather than his beauty, which wins the day." Here, again, vigour and intense vitality seem to be the chief recommendations of the male in the eyes of the female, and these—as is very strikingly manifest in the game cock—appear correlated with intense colouration. Mr. Wallace resumes:—"Evidence collected by Mr. Darwin himself proves that each bird finds a mate under any circumstances. He gives a number of cases of one of a pair of birds being shot, and of the survivor being always found paired again almost immediately. This is sufficiently explained on the assumption that the destruction of birds by various causes is continually leaving widows and widowers in nearly equal proportions, and thus each one finds a fresh mate; and it leads to the conclusion that permanently unpaired birds are very scarce, so that, speaking broadly, every bird finds a mate and breeds. But this would almost or quite neutralise any effect of sexual selection, of colour, or ornament, since the less highly-coloured birds would be at no disadvantage as regards leaving healthy offspring." Whilst accepting this conclusion, we may ask whether the same argument is not capable of further application? It is generally stated that the "fittest" male—*i.e.*, the one most in harmony with the circumstances in which he is placed—will have the best chance of securing a mate and of leaving offspring, whilst the feebler, the slower, the less energetic, and those least in harmony with the situation, will be left in a state of single blessedness, and will not transmit their attributes to posterity. But on the principles laid down in the passage we have just quoted the effects of natural selection will be greatly neutralised. It must, however, be remembered that the destruction of birds, especially in a state of Nature, will not fall exclusively or mainly upon those which have secured mates, but will likewise extend to the unwedded.

Whilst combatting Mr. Darwin's view, that the brilliant colours of butterflies have been acquired for the sake of protection, Mr. Wallace remarks:—"It is in fact somewhat remarkable how very generally the black spots, ocelli, or bright patches of colour are on the tips, margins, or disks of the wings; and as the insects are necessarily visible while flying, and this is the time when they are most subject to attacks of insectivorous birds, the position of the more conspicuous parts at some distance from the body may be a real protection to them." This rule, however, is by no means universal. The fire-wasp (*Chryseis*) and not a few other Hymenoptera have brilliantly-coloured bodies, but colourless and transparent wings, which when expanded and in action are scarcely visible. In numbers of Lepidoptera the more intense colours, especially reds, are found entirely or mainly on the posterior wings, which extend to a less distance from the body than do the anterior pair. In many cases again Lepidoptera, Coleoptera, and Hymenoptera display conspicuous colours at the extremity of the abdomen, where a blow from the beak of a bird would doubtless permanently disable.

A question may here arise concerning the use of the colouration of the posterior or true wings in certain beetles, a circumstance not sufficiently examined. Whilst these wings in the vast majority of Coleopterous species are colourless, or at most of a very faint yellowish hue, in the Colorado beetle they are pink, and purple in several *Chrysochroas*, *Pachnodas*, and *Lomapteras*. Why should these species thus differ from other closely-allied forms, with whom they appear to agree most closely in their habits?

We have no doubt that Mr. Wallace's formal declaration against the doctrine of Sexual Selection will attract the attention of disbelievers in Evolution, and we venture to hope that all the comments which will be elicited may not be beside the question.

IV. ON THE DISCOVERY OF STONE IMPLEMENTS IN GLACIAL DRIFT IN NORTH AMERICA.

By Thomas Belt, F.G.S.

THE discovery of great numbers of stone implements in New Jersey, by Dr. C. C. Abbott, in deposits which are probably of Glacial age, is of such great importance that a detailed account of the beds in which they have been found and a discussion of their antiquity will be interesting to many. I had, during the past autumn, an opportunity of studying these beds under the kind guidance of the discoverer of the implements; and I am also indebted to Prof. Cook and Prof. Smock, of the Geological Survey of New Jersey, for much information respecting the glaciation of the State. I shall, in the first place, give a brief statement of what was before known of the earliest traces of man in North America.

Before these discoveries there had been many intimations of the great antiquity of man in the western hemisphere. Probably one of the earliest of these was the discovery of the fragment of a human bone which was said to have been found at the base of 60 feet of loess, near Natchez, on the Mississippi, along with the remains of the megalonyx and other extinct quadrupeds. A full description of the deposits in which these remains were discovered has been given by Sir Charles Lyell, in his "Second Visit to the States."* We learn there that Dr. Dickeson, of Natchez, felt persuaded that the fragment of human bone had been taken out of the clay underlying the loam; but Sir Charles Lyell could not ascertain that it had been actually dug out in the presence of a geologist, or any practised observer, and he speculated on the possibility of it having fallen from above, into the bed of the ravine, from some old Indian grave. This was in 1846: long afterwards, when the discoveries in Europe had established the contemporaneity of man and the great extinct pachyderms, he recalled the fact that the human bone was in the same state of preservation and of the same black colour as the bones of the mastodon and megalonyx, said to have been found with it; and he was disposed to think that he had discussed its probable age with a stronger bias, as to the antecedent improbability of the contemporaneous entombment of man and the mastodon, than any geologist would now be justified in entertaining. *

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* *Op. cit.*, vol. ii., p. 196.

The fragment of a human skull from Calaveras, in California, which was said to have been found in gravel beneath five successive overflows of lava, would, if authenticated, be probably the oldest record of man in North America. The same doubts, however, have been expressed about it as about the Natchez remains, no geologist being present when it was exhumed. In the newer gold-drifts of California, along with the remains of the mastodon, elephant, tapir, bison, and horse, the implements of man have been frequently found.†

In the auriferous gravels of Kansas and Georgia stone and flint implements have also been discovered.‡

Dr. Samuel Aughey, in his account of the superficial deposits of Nebraska, states that the remains of elephants and mastodons are often found in the loess that overspreads nearly the whole of the State. In this deposit, in a railway-cutting near Omaha, 20 feet from the surface, he dug out himself a large coarse arrow- or spear-head which lay 13 inches below the lumbar vertebra of *Elephas americanus.*‖

Near Alton, in Illinois, stone axes and flint spear-heads along with the bones of the mastodon are reported from drift below loess.§

All the above discoveries are in regions that drain either into the Pacific or the Gulf of Mexico.

Mr. Chas. M. Wallace has described the discovery by him of flint implements in stratified drift near Richmond, Virginia.¶ These deposits seem to be similar to those in which Dr. Abbott has made his discoveries in New Jersey. The valley of the James River is mantled by thick deposits of coarse gravel covered with brick-clays. The implements have been found occasionally in the clay, and more frequently in the underlying gravel. One of the sections given by Mr. Wallace shows the following succession of beds:—

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* Antiquity of Man, first edition, p. 200.

† J. D. Whitney, Geol. Surv. California, vol. i., p. 252.

‡ Dr. D. Wilson, Canadian Journal of Science, October, 1877, pp. 559, 560.

‖ Geol. Surv. of the Territories, 1876, p. 254.

§ Geol. Surv. Illinois, 1866, vol. i., p. 38.

¶ Amer. Journ. Science, March, 1876, vol. xi., p. 195.

	Feet.
Brick earth underlying greyish clay ...	9
Rounded gravel, reddish hue	4
Fine bluish sand...	12
Gravel and bluish pebbles...	4
Compacted sand (probably Tertiary).	

Several implements were found on the surface of the lower bed of gravel. This lower gravel contains large numbers of the pebbles from which the implements, for the most part, appear to have been fashioned. In some parts large boulders (one 8 ft. by 12 ft.) rest upon the gravel, and appear as if they had been brought by floating ice and deposited in gentle waters. Mr. Wallace notes the similarity of many of the implements to those of palæolithic age in Europe. I believe this is the first notice of the discovery of palæolithic implements on the eastern sea-board of North America,

The report by Dr. C. C. Abbott of his discoveries of stone implements in the drift-gravels near Trenton, New Jersey, appeared in the "Tenth Annual Report of the Peabody Museum," issued during the present year.* My attention was drawn to it, soon after its publication, by Dr. D. Wilson, of Toronto,—who has since reviewed Dr. Abbott's paper,†—and in consequence I visited the locality. Dr. Abbott showed to me a great number of the implements he had found, and afterwards accompanied me to the principal places near Trenton from which they had been obtained.

Whilst a few of the implements resemble some of the palæolithic chipped flints of England and France, the general form and type is of a ruder and more imperfect character. Some are simply made from rounded flat pebbles by chipping a cutting edge at one end. Amongst them are many of what Dr. Abbott has named the "turtle-back" type. It appears to have been formed by using a pebble with one side naturally flat, or by producing a flat surface by artificial fracture and bevelling down the other side by chipping, so as to produce a cutting edge.

Whilst the general character of the implements is ruder than the European, a few appear more like a spear-head than I have seen amongst the latter. I have shown a few, that Dr. Abbott liberally presented to me, to our greatest authority on stone implements, Dr. John Evans, and he considers they are different from the palæolithic type in Europe, and more resemble some of the ruder neolithic implements from Ireland. It is surprising that palæolithic implements from distant parts of the Old World resemble each other so closely, and it would have been more wonderful if those from America had been fashioned to the same type.

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* *Op. cit.*, p. 30.

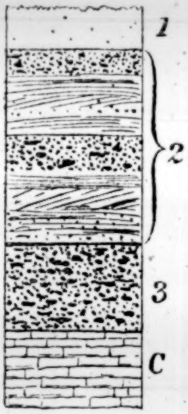
† Canadian Journal of Science, October, 1877, p. 557.

Most of these implements are from the high bluff facing the Delaware, near Trenton; some from other localities in the same valley. Amongst them is one which Dr. Abbott picked out himself from the face of a railway-cutting through a boulder deposit at Butzville, 50 miles north of Trenton. It is made from a flattened pebble, by one end being chipped to a cutting edge, the other being left in its natural rounded condition. Both on the rounded portion of this specimen and on the chipped part are what appear to be glacial scratches. Dr. Abbott informed me that nearly every stone in this deposit was covered with glacial striæ, and Professor Smock afterwards told me that the formation had been recognised by the Geological Survey as part of the terminal moraine of the great ice-sheet. I felt no doubt when I examined this specimen that it had been fashioned by man, but Dr. Abbott has since informed me that others to whom he has shown it think that it is barely possible that it may be of natural formation. I fully expect that it will be authenticated by further discoveries, but in the meantime it may be well not to base any theories upon it. I noticed small scratches upon some of Dr. Abbott's other specimens, and he kindly presented one to me showing these on one side. As I shall have to explain further on, the glacial age of at least some of these implements can be proved without reference to the one from Butzville, or whether the scratches on others are glacial or not; and I am disposed to place less value on the striæ on the Trenton specimens, because it is obviously possible that they may be artificial.

Opposite Dr. Abbott's house, about 2 miles below Trenton, the high bank bounding the river has been worn back into a deep bend, and a wide alluvial plain now lies between it and the Delaware. I sketched the section of the beds forming the bluff (Fig. 1) at this point. The top bed is here an unstratified sandy clay, with a few scattered pebbles, and occasionally very large boulders, none of which were, however, seen at this locality; beneath this lie alternations of fine sandy gravel, sand, coarse gravel, and boulder-beds. Dr. Abbott pointed out to me the upper layer of pebbles as the horizon from which he had obtained some of the implements in undisturbed ground.

Towards Trenton the bluff approaches the river, and just below the town forms its bank. The face of the bluff is mostly a talus; there are few good sections, and none that I saw were perfectly satisfactory. The best section was near the cemetery, where I made the sketch shown in Fig. 2. The lower bed (No. 3) is quite unstratified; only the upper 12 feet of it is seen where the section was taken, the lower part being covered with talus, but in other places

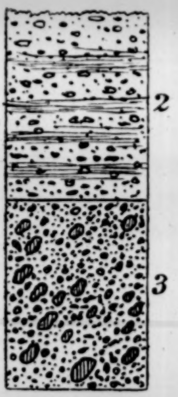
FIG. 1.



1. Sandy clay, 3 to 6 feet, with large boulders on surface in some places.
2. Alternations of gravel and sand, occasionally false bedded, 6 to 10 feet. Stone implements.
3. Unstratified pebble and boulder bed. Thickness very variable.

C. Cretaceous marls and clays.

FIG. 2.



2. Irregularly stratified sand and gravel, with occasionally larger stones intermixed, about 12 feet. Stone implements.
3. Thick unstratified bed of pebbles and boulders, mostly rounded, with many stones up to 15 inches diameter; larger ones rare. Base of deposit not seen.

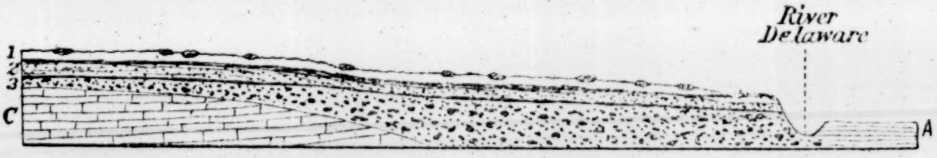
it was seen going down to and below the river, the surface of which is about 40 feet below the top of the bluff.

The irregularly stratified beds, No. 2, contain generally much smaller stones than No. 3. These beds, as in the former case, were pointed out to me by Dr. Abbott as the ones from which he had obtained the implements, and whilst I was present he discovered a rude one in the talus which appeared to have come from them, as we were too high up the slope for it to have come from the lower bed.

The bed of sandy clay (No. 1 in Fig. 1) has been denuded from the top of the bluff, but a little way back from the edge it appears, and contains great boulders scattered over its surface.

From all the sections that I saw, and from the information given to me by Dr. Abbott, I have constructed the general section (Fig. 3) showing the succession of the beds. Up to the time of my visit (October, 1877) no implements had been found in the lower quaternary bed (No. 3), though it seems to me extremely likely that they will yet be discovered. This bed, of mostly rounded boulders and large pebbles, is quite unstratified. Amongst the pebbles are many flattened ones, similar to those from which some, if not all, the implements found in the higher beds have been made. This rounded boulder-bed is of great extent, and has been described by Prof. W. B. Rogers[*] as extending from the Delaware into Virginia. At Washington the deposit covers the whole plain on which the city is built, rising to a height of about 200 feet above the sea over the low hills around it. Prof. Rogers has found in it stones containing *Scolithus linearis*, a well-known fossil of the Potsdam formation, having its nearest outcrop on the western side of the Blue

FIG. 3.—DIAGRAM SECTION BELOW TRENTON.



1. Sandy clay, unstratified, with a few pebbles, and with very large ar-transporte boulders on or immediately below the surface.
2. Irregularly stratified sands and gravels. Stone implements.
3. Pebble and boulder bed. Stones up to 15 inches across plentiful; larger ones rare. Stones mostly rounded, with a few subangular ones; no scratched ones seen.

A. Alluvium.
C. Cretaceous marls and clays.

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[*] "On the Gravel and Cobble-stone Deposits of Virginia and the Middle States." Proc. Bos. Soc. Nat. Hist., 1875, vol. xviii., p. 101.

Ridge. Some of the stones found near Richmond must have been brought 80 miles if they travelled in a direct line from the parent rock. At Richmond this deposit is said to present itself at various heights from the river-bank to the tops of the hills, mantling the irregularly denuded surface of the underlying formations, resting at one place on the Upper Miocene, at others on the Eocene, or yet older deposits.*

At Trenton this deposit is principally composed of crystalline rocks, many of distant origin, but none I believe have been noticed that may not have been derived from formations existing within the drainage area of the Delaware and its territories. Trenton is often mentioned as being about the southern limit to which the northern ice reached in the valley of the Delaware in glacial times, but I could find no traces of glaciation in the neighbourhood ; and Prof. Cook informed me that it has not been noticed farther south in the valley than near Belvidere, about 50 miles in a direct line N.N.W. from Trenton. Prof. Cook and Prof. Smock have now traced the southern boundary of the land ice pretty clearly across the State of New Jersey, from the neighbouroood of Amboy, on the Atlantic coast, to Belvidere, on the Delaware. From thence it runs across to near Harrisburg, in the valley of the Susquehanna, where Prof. J. P. Lesley informed me were the most southern glaciated rock surfaces in that valley. North of an undulating line passing through these points the surfaces of the bed-rocks are rounded and polished, and scored with glacial striæ. By means of these markings, and by the direction from which transported rocks have been brought from their parent beds, the course of the ice has been mapped out from the Canadian boundary, where it was so thick as to be able to over-ride and move independently of the valleys, up to its southern termination, where it is found conforming with the direction of the main drainage channels. South of the line to which the northern ice extended the rock surfaces are often decomposed to a great depth. This is especially evident where the bed-rock is gneiss or granite. In such cases, for more than 50 feet from the surface, the rocks have often been changed to a clay that may be dug with a spade ; whilst that it has not been otherwise reconstructed is evidenced by the fact that veins of quartz running through it remain in their original position.† Over the glaciated district, on the contrary, the decomposed crust has been removed, and the hard unaltered rock laid bare.

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* *Ibid.*, p. 106.

† See T. Sterry Hunt, "Decomposition of Crystalline Rocks.' Amer. Journ. Sci., 1874, vol. vii., p. 60.

It has been shown that the decomposition of the rocks has been caused by the slow percolation of rain-water containing a little carbonic acid. It follows that the surface rocks had been exposed for long ages to this influence, and that the time that those of the glaciated districts have been subjected to the same action, since the Glacial period, is exceedingly short in comparison.

Another mark by which the glaciated may be distinguished from the non-glaciated country is by the occurrence of a deposit long known in Scotland under the name of "till," and on the Continent as "grund morane" and "moraine profonde." It is generally a stiff clay, full of small angular fragments of the rocks over which the ice has passed, and sometimes with large angular and subangular stones. It always more or less reflects the characteristics of the strata immediately in the neighbourhood and in the direction from which the ice has come. Thus in the vicinity of Toronto I noticed, as Mr. G. J. Hinde had before remarked,* that the till is packed with small fragments of black Utica shale and blue Trenton limestone—strata that the ice had passed over in it's passage from the eastward. Along with these were larger fragments and slabs of the underlying Hudson River group, and a few rounded boulders of gneiss that were far travelled. Around New York many patches of till are left on the glaciated rocks. I visited Marion, near the city of New Jersey, by the advice of Prof. Cook, and found very fine sections of the glacial beds. The till is there not a stiff clay, but a rather sandy deposit, of a dark reddish brown colour, packed with the angular *débris* of the red triassic sandstones that form a large portion of the bed-rocks of eastern New Jersey. A few of the contained stones may have been brought from a distance, but the great bulk of them, as well as the sandy matrix, are of local origin.

The deposition of the till probably took place during the melting back of the ice-sheet. Dr. Dana has shown that the ice was very thick over New England, and that the pressure at its base would be so great as to force the plastic mass into the crevices of the rocks below, so as to tear off fragments from them, which, with any loose material it met with in its progress, would be gathered up and borne along in the lower portion of the ice-sheet.† Prof. Joseph Le Conte, in his study of the deposits left by the ancient glaciers of the Sierra Nevada,* has arrived at a similar conclusion. Prof. Hall, in his "Geology of New York," has given a large picture of a natural section on the shore of Lake Erie, in Chautauque county, where we may see as it were the till in course of formation. In some parts the top strata are only a little separated, with clay forced in between them ; in others they stand on edge, or are broken up and the fragments scattered throughout the till.† I have seen similar instances on the shores of Lake Ontario. In our own country there are also many examples of the same action ; one of the finest, that I have seen, being in the beautiful section of the glacial beds exhibited in the cliffs at the mouth of the Tyne, in Northumberland. In this way the lower portion of the ice appears to have become charged with stones and finer materials which were left on the surface when the great glacier melted back. The till is therefore restricted to the area that the land-ice covered, and is as much a memorial of its former presence as the scratched and rounded rocks on which it generally lies.

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* Canadian Journal, April, 1877, p. 8.

† "On the Glacial and Champlain Eras in New England." Amer. Journ. Sci., March, 1873.

By the glaciated rock-surfaces up to the line I have already mentioned and their absence beyond, by the outspread of the till limited by the same boundary, and by the disappearance of the decomposed surface-rocks up to but not beyond that margin, we know that the land-ice did not reach, in the valley of the Delaware, farther than the neighbourhood of Belvidere, which is 50 miles to the north of Trenton. What relations, then, do the beds of drift that I have described at Trenton bear to the ice-sheet ? To answer this question we must take into consideration what we know is taking place at the terminations of existing glaciers. Great streams of water run from underneath them, bearing along fragments of rocks that have been melted out of the glacier or fallen through crevices to the subglacial rivers. These are rounded by attrition, and spread out in sheets of pebbles often extending for miles below the terminations of the glaciers. From the enormous glacier that once filled the Delaware valley as far as Belvidere, and which was itself only a southern prolongation of the still greater northern ice-sheet, there must have been shed a vast amount of similar material. The large rounded drift that lies at the base of the quaternary beds at Trenton (No. 3 in Figs. 1, 2, 3) is almost undoubtedly a similar deposit. It contains a mixture of all the various rocks over which the glacier of the Delaware would pass; those from the hardest formations being most abundant, as they would best survive the severe abrasions to which they had been subjected.

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* Amer. Journn. Sci., 1875, p. 126.

† *Op. cit.*, Plate VIII.

No implements are yet reported from this bed, so that, putting aside the disputed implement from the morainic accumulation at Butzville, we have no evidence at present that man frequented the valley of the Delaware before or during the greatest extension of the glacier of that valley. The deposits from which Dr. Abbott has obtained the implements lie above this great unstratified bed of rounded drift, and we should have great difficulty in fixing the approximate age of the implement-bearing strata if it were not for the fortunate occurrence of the sandy clay with large boulders (No. 1 in Figs. 1 and 3) clearly superimposed on the latter. We may now turn our attention to the consideration of this surface-bed and the relation it bore to the Glacial period.

In my discussion of the glacial and post-glacial phenomena bearing on the date of the excavation of the gorge at Niagara, published in this Journal,* I have described the occurrence of large boulders of crystalline rocks lying above all the other glacial beds. In the till which lies next the glaciated bed-rocks the stones are all of local origin; in the surface deposit they are all from the distant north.

Prof. James Hall, so long ago as in 1843, had fully recognised the importance of the occurrence of these far-transported blocks that lie scattered over the surface, and had noted the difference in the mode of their occurrence and in their composition from the rocks included in the lower glacial beds.† He shows that the glacial beds belong to two periods: one, the lower, which contains mostly local rocks; the other, the upper, containing far-transported crystalline rocks. He says that on the broad northern slope towards Lake Ontario, where hills are distant, there are numerous and extensive fields of boulders resting upon the surface, or but partially imbedded in the soil, and holding such a position that it is evident that they are of subsequent origin to the great body of detritus; and again, on the western prairies, long lines of boulders are to be observed stretching away for miles beyond the reach of vision, as if once forming a line of coast or deposited along some channel or course of a current, though the general surface indicates no influence upon this portion beyond what is common to the whole. Prof. Hall considered that there was no explanation of the transport of these great blocks, excepting on the supposition that the whole surface was covered with water, over which they were floated on icebergs. "Had they been transported," he says, "by a powerful current over the bottom (which cannot be supposed from the inequalities of the surface) all the older drift would have been removed at the same time, and instead of finding them as we do now, mostly upon the surface, they would have been imbedded indiscriminately in the superficial detritus, and there would have been no means of recognising the products of different periods."*

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* *Op. cit.*, April, 1875.

† Geology of New York, Part IV., pp. 319 to 321.

Dr. Newberry, in his "Surface Geology of Ohio," has fully described the distribution of large boulders over the surface of that State. Even in Southern Ohio they are in some parts very numerous. He says that the large unscratched boulders are generally found on the surface, and that in the great series of excavations, which have been made in the construction of the railways and canals, they have been rarely met with below it. They are often seen resting on the fine stratified clays which form the upper part of the drift. And he observes that "it seems impossible that they should have been brought to such positions by glaciers or currents of water, as either of these agents would have torn up the underlying clays. We also learn, from their relative position, that these boulders were deposited at a later period than the most recent stratified beds of the drift series, and that they were floated to their present resting-places. In short, no argument is required to convince anyone who will glance at the facts that these boulders, and probably the gravel and sand with which they are sometimes accompanied, were floated on icebergs from the north shore of the great fresh-water lake which once filled the lake basin, and that as these icebergs melted, or when they stranded, their loads were discharged on the top of all the drift deposits which had been laid down in the preceding epochs of the Quaternary age."†

On the eastern side of the Appalachians, Prof. Hall has noticed the occurrence of these boulders in the valley of the Hudson, and says that he has searched in vain, near Albany and Troy, for a boulder or pebble of granite, or of any rock older than the Potsdam sandstone in the deposits below the clay; while in a period subsequent to the deposition of the clays and sands, boulders of granite are by no means rare.*

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* *Ibid.*, p. 336.

† Surface Geology of Ohio, 1874, p. 40.

In the southern part of the State of New York and in New Jersey they are not uncommon. At Marion, at the section of the till to which I have already referred, the top bed is a light-coloured sandy clay, similar to that at Trenton. Lying on and sometimes imbedded in this are large boulders, scattered over the surface. The sandy clay rests directly on the till, and is about 3 feet thick. Both here and at Trenton these great boulders were much larger than any I saw in the underlying till or drift. At Trenton they are often seen in the formation of new streets on the outskirts of the town. Some of them are 7 or 8 feet across, and most require blasting before they can be removed. I learnt from Prof. Smock that these blocks are distributed over much of the State, and he spoke of particular boulders occurring at a considerable altitude. I do not know, however, how high they occur, but probably this interesting question will be worked out by the Geological Survey of New Jersey, as well as the distances which they must have travelled from their parent rocks.

Nor does the Delaware form the southern limit of the far-transported boulders. They appear to bear the same relation to the drift-beds in Virginia, for Mr. Wallace, in his account of the discovery of stone-implements near Richmond, speaks of boulders in the surface-soil, and of large blocks (8 feet by 12) resting on the gravel.

It is obvious, as Prof. Hall and Dr. Newberry have pointed out, that these great blocks of stone must have been carried to their present position by floating ice. Any flood of water sufficient to move them would certainly wash away the sandy loam in and on which they rest, and such a mode of transport would not account for their position scattered here and there over the great undulating plain that extends from Trenton to the sea; nor could they have been left by the great ice-sheet, as they are found far beyond the limits to which it reached. Sometimes we hear the distribution of the upper glacial beds ascribed to a second Glacial period, when the ice again covered the land. But ice could not have moved thus for hundreds of miles over beds of gravel and sand without disarranging them, and nowhere in America has any sign been noticed of a second advance of the northern ice. We have thus two distinct phases of the glacial era clearly marked in North-eastern America,—a Glacier period and an Iceberg period, just as we have in Europe. They are distinct in their range and distinct in their effects. In the first—the Glacier period—the ice, moving gradually southward, scored and polished the rocks over which it passed, and left behind it the unstratified till containing principally scratched fragments of local rocks. In the other—the Iceberg period—rocks were carried many miles beyond the limit that the glacier ice reached to, and were dropped on the top of loose unconsolidated clays and sands, which show no trace of any abrading or disturbing force. In Europe the ice from the Scandinavian mountains reached to the southern side of the Baltic, and for the whole distance the bed-rocks are glaciated; but beyond this the iceberg drift is scattered for hundreds of miles, and extends to the flanks of the mountain-chains that bound the German plain to the south; and that icebergs do not, as a rule, glaciate the beds over which they pass may be gathered from this,—that as soon as the boundary is left behind to which the land-ice undoubtedly reached no more glaciated rock-surfaces are seen; not even on the hills on which the icebergs must have grounded, as they have left there the greater part of the rocky burden they carried.

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* Geology of New York, Part IV., p. 319.

The agency of floating ice in the distribution of boulders was early recognised by geologists; but when, later on, Agassiz proved that land-ice had also played a most important part, it was not clearly perceived that both agencies were required to interpret the phenomena, and to this day the till—the product of the land-ice—is often confounded with the boulder clay, the product of the floating ice. In no other department of geology is far-travelled experience more necessary than in the study of the glacial beds. The knowledge to be acquired in a single province, or even in a single country, is not sufficient, for it will be well nigh impossible from that alone to separate what is particular and local from what is widespread and general. To limited experience I cannot help believing is due the obscurity to be observed in many of the memoirs dealing with glacial problems. One authority, who has perhaps lived amongst northern mountains, ascribes everything to the action of glaciers; another, whose home, maybe, has been on southern plains, sees nothing but the agency of water and floating ice.

In studying the glacial beds of North-eastern America we must seek to give their proper importance both to glaciers and icebergs, and to separate the phenomena into the two classes to which they belong. When we do this we find, as I have endeavoured to show, that the land-ice came down from the north to a certain well-defined line in New Jersey and Pennsylvania, and that after it melted back the country was submerged beneath a great expanse of water that covered the whole of the lower ground and reached far up the flanks of the hills, and that over this icebergs floated from the north, and dropped, as they melted, large stones brought from far distant ranges. This expanse of water was not limited to the area that the land-ice had covered, but extended far to the south of it into Virginia. After the land-ice retired, or whilst it was retiring, and before the country was submerged to such a depth as to permit the flotation of icebergs from the north, the upper pebble beds containing the stone implements were formed. Dr. Abbott has not only obtained his implements from beds that are clearly seen to have been spread out before the large blocks were scattered over the surface, but in one instance took one from the gravel below one of the large stones. From Mr. Wallace's description his discoveries appear to have been made in gravels of the same age.

West of the Appalachians the evidence all points to the same conclusion. We have in the Northern States, first, glaciated rock-surfaces and patches of till that witness the reign of land-ice; then we have on its retirement a land-surface, with remains of vegetation (peat and forest beds) and of extinct mammals. Along with the latter at some places, at the same horizon in others, have been found the bones and implements of man, as I have described at the commencement of this paper. The next stage is marked by widespread beds of gravel or rolled drift, that indicate the rising of the water. The gravel is covered with brown clay containing great far-transported boulders, witnessing the submergence of nearly the whole country beneath the flood. This brown clay covers the land everywhere in the States of Illinois, Iowa, Kansas, and Nebraska, and I have traced it myself up to the flanks of the Rocky Mountains. It marks as surely the culmination of the great flood as the beds that follow it, the loess, mark its subsidence; when the waters that had before covered the hills began to be confined to the valleys of the great rivers. From this time the mammoth, the megalonyx, the megatherium, the mylodon, the horse (until it was re-introduced from Europe), the gigantic beaver, and the lion were no more seen alive in North America, for their remains are not found in beds of later age. To the same horizon belong all the instances I have given of the earliest appearance of man in North America, and to it almost certainly must be ascribed the discoveries of Dr. Abbott and Mr. Wallace on the eastern sea-board. The extinct mammals and the earliest appearance of man in North America are therefore prediluvial, as I have urged is the case in Europe: indeed a most striking parallel may be drawn between the series of events that happened in the Glacial period in the eastern and the western continent.

We have first in Europe a great extension of land ice, that from Scandinavia reaching to the south of the Baltic, and that of the Alps to the Jura and down the valley of the Rhone as far as Lyons. We have then the retreat of the ice; and palæolithic man, the mammoth, and the rhinoceros occupying part at least of the area the ice had covered. Then we have a great outspread of gravels and clays, the latter in Northern Europe, with far-transported boulders, reaching up to 1700 feet above the present level of the sea.

I have endeavoured to explain this series of events by the theory that whilst the ice was accumulating on the mountains of Scandinavia and Central Europe, it was also being piled up at the northern end of the Atlantic, and in greater abundance there because of greater precipitation. When it there reached a sufficient height to intercept the moisture in the air-currents travelling northward it would advance down the bed of the Atlantic, partly by flowing as a glacier, but principally because the precipitation was on the southern slope and increased as the ice-ridge progressed southward. Whilst this ridge of ice was moving down the bed of the Atlantic, that from Scandinavia and the Central Alps had culminated, and began to shrink back, for the area of greatest precipitation was now on the Maritime Alps, the Pyrenees, the Cantabrian Range, and the mountains of Asturias, and the accumulation of ice there intercepted the moisture that had before supplied the glaciers of Northern and Central Europe. This I consider was the time of the principal distribution of the mammoth and the woolly rhinoceros, an earlier stage being marked by the presence of *Elephas antiquus* and *Rhinoceros etruscus*. The great accumulations of ice in the northern and southern hemisphere had before this abstracted so much water from the ocean that the level of the latter had been greatly lowered, the rivers cut deeper channels than they now occupy, the bed of the German Ocean was left dry, and many another tract that is now covered with a shallow sea then formed wide pasture-grounds for the great pachyderms and their associates. Palæolithic man likely lived on higher and drier land, and found a plentiful subsistence amongst the deer and the wild horses and oxen that appear to have abounded.

Probably this state of things was of long duration, but at last there came a catastrophe; possibly the greatest that has befallen the human race, yet of immense benefit in its ultimate results. The ridge of ice in the Atlantic had been slowly advancing, and through time it coalesced with that from the Cantabrian Range, whilst at the same time the gap between the Pyrenees and the Maritime Alps was also closed with ice. I suppose that the communication of the Black Sea with the Mediterranean had not then been effected, and that the ice of the Pacific blocked up the outlet of the waters to Behring's Straits. An immense basin was thus formed, the drainage of which to the sea was intercepted. The consequence would be that the low lands would be soon submerged. The pent-up waters ultimately reached a height of about 1700 feet above the sea, as evidenced by the great outspread of gravels at Munich, Bern, and Geneva; but whether this extreme height belonged to the first or second rise I do not yet know. To this great flood I ascribe the formation of the lower boulder clays and diluvium, and the destruction of the great mammals that were caught on the low plains and have left there their bones in great abundance. The first great European lake was apparently not of long duration, but was suddenly and tumultuously lowered by the breaking away of the ice-dam; probably, I now think, between the Pyrenees and the Maritime Alps. The rushing flood or debacle swept off from the flanks of the hills much of the detritus that covered them, and mingled all together in the great sheets of gravel that are now spread over much of the low country. Thus were formed, I think, the middle sands and gravels (including the Thames and other valley gravels), in which have been caught up or which cover the bones of the pre-diluvial mammals or the stone implements of pre-diluvial man. As no land surface has yet been detected between the middle sands and gravels and the upper boulder clay, it is probable that the break in the rim of the lake basin was soon filled with ice again, and the great lake re-formed. Over it floated icebergs from the north, carrying great boulders from the mountains of Scandinavia and scattering them over the German plain and as far as the flanks of the Carpathians. At this time was formed the upper boulder clay and diluvium of Europe. The second lake was gradually lowered by the cutting through of the Bosphorus or the Dardanelles, and the various stages of its subsidence are marked in all the great valleys of Northern and Central Europe. In England the upper boulder clay and the upper brick clays of the Thames and other river valleys were at this time deposited. Flint implements appear to have been found in clays of this age, but they do not indicate, I think, that palæolithic man existed, but that pre-diluvial man had left his nearly indestructible stone-work on the hill-sides, higher than the violence of the debacle reached to, and that shore-ice in the second-lake period sometimes carried these away and dropped them in the clay that was then forming. I noticed, in Dr. John Evans' noble collection of stone implements, with surprise, a fact with which he had been long familiar—the sharp, unworn edges of the implements from the brick clays, and also of a few that have been found on the surface at heights of over 300 feet above the sea. These implements have also a whitened bleached appearance, which may be due to long exposure on the surface before being imbedded in the brick clays.

It is now more than two years since I laid this theory before the Geological Society of London,* and no flaw has yet been pointed out in it, whilst in a series of papers published in this Journal I have shown that many other difficult problems in glacial geology besides those to the solution of which I first applied it find in it a simple explanation. I believe it is the only theory that explains the transport of northern boulders across the plains of Germany and Russia; and at the same time accounts for the absence of marine remains testifying that the sea had not occupied the area during the flotation of the blocks; and the absence of glaciated rock-surfaces showing that the Scandinavian land-ice had not extended so far. It is also the only theory of our day that deals with the difficult question of the origin of a great debacle of which De la Beche, Murchison, Sedgewick, and Prestwich have shown us there is so much evidence. The principal feature in the theory is that the advance of the ice of the Glacial period was mostly down the ocean depressions, partly because ice will gravitate towards the lowest levels, and especially because the precipitation of moisture is in our hemisphere much greater at the northern ends of the seas than in similar latitudes inland on the continents.

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* "Drift of Devon and Cornwall." Read November 3rd, 1875. Published in abstract only, Quart. Journ. Geol. Soc., February, 1876.

If the northern end of the Atlantic was so occupied with ice as this theory requires, the effects ought to be similar on the west coast of Europe and the east coast of America, which on this view form the left and right banks of the same great valley. I restrict my argument for the present, on the American side, to the country lying east of the Appalachians, but I hope at some future time to show that a similar explanation of the glacial phenomena west of that range is not improbable; this I cannot do now, as the preliminary steps of the discussion would occupy a greater length than the whole of this paper.

Owing to the influence of the Gulf Stream the ice occupying the bed of the Atlantic would probably extend much farther on the American side than on the European. Flowing down there—much influenced by the shape of the ocean bed, still more by the areas of greatest precipitation as affected by the advance of the ice itself, and not necessarily, nor even probably, thickest next the coast line and south of Cape Cod mostly distant from it—the ice, I think, reached so far at least as the 37th parallel of latitude. I suppose that the mass of ice had been increasing as it advanced southward, in consequence of the enormously greater precipitation not having yet been counterbalanced by the also increased waste from liquefaction, and that it flowed in upon the American coast somewhere south of Chesapeake Bay, and blocked up the eastern drainage as far as that point. Thus I think was produced the submergence of all the lower parts of the country. To what height the flood reached I have not information to guide me, but the water must have been deep to permit the tranquil deposition of the brown clays that cover much of the country, and the flotation of icebergs from the north, bearing the great rocks that were thus distributed over the land. I have found no evidence in North America of any great debacle, and the waters do not appear ever to have been suddenly and tumultuously discharged. In consequence, there has not been there the same mixing together of remains of different ages as occurred with us when the middle sands and gravels were spread out, and the relation of the beds containing the relics of pre-diluvial man and the pre-diluvial mammals to the other glacial deposits is more clearly defined. The more gradual and interrupted subsidence of the water is, however, marked by a series of terraces in the valleys. Excepting for this, the parallel between the series of events that occurred in the Glacial period, in Western Europe and North-eastern America, is complete. There is the same evidence of the advance of the land-ice, of the rivers running in deeper channels, of the formation of forest and peat beds in tracts now below the level of the sea, of the recession of the land-ice, of the interruption of the drainage of the country, and the formation of a continental ice-dammed lake over which floated icebergs. The same evidence, too, that palæolithic man and the extinct mammals were pre-diluvial, and were destroyed or driven out of the country by the rising of the great flood.

That American geologists will follow up the evidences of pre-dilvuial man in the western hemisphere we may be sure, and we may confidently expect that as great advances will be made by them in our knowledge of the relation he bore to the Glacial period as they are making in every other department of geology, and in fact in every branch of science.

It is a matter for congratulation that this question should be in the hands of such a skilled and enthusiastic archæologist as Dr. Abbott, and of such able and cautious geologists as Prof. Cook and Prof. Smock. I feel confident that we shall not have to wait long for confirmation of the position of the implements below the iceberg drift, and for more definite information than we now possess of the height above the sea to which the erratic blocks extend, and the distances they have travelled from the north or north-west. Nor need we despair of evidence soon being found that man was present in the country at the time of the greatest extension of the land-ice; the witness of which, so far, is the solitary scratched chipped pebble from the moraine at Butzville, the fabrication of which by man is doubted by some that have seen it.

I cannot conclude this brief view of the broad features of the glaciation of North-eastern America and the relation of palæolithic man to it, as seen from my standpoint, without again making an appeal for a more thorough examination of the records in our own country. It is susceptible of proof in East Anglia whether or not palæolithic man lived there in the Glacial period. Within a stone's throw at Hoxne lie all the glacial beds—the till, the lower boulder clay, the middle sands and gravels, and the upper boulder clay. There also are the gravels and clays in which Mr. Frere, nearly eighty years ago, found flint implements and bones of extinct mammals; and yet to this day we have not settled the relation that these bear to the glacial beds. Eighteen months ago, in the pages of this Journal, I gave my reasons for believing that the post-glacial age of these deposits, as assumed by most of our geologists up to that time, had not been proved; and I urged that we ought not to allow the matter to remain doubtful when it could be cleared up by sinking a few shafts in different parts of the ground. No action has been taken by our learned societies to whom I appealed, but now discoveries in other places have caused many to doubt the post-glacial age of some of the deposits containing implements, and they may be more inclined to listen to my appeal.

At Hoxne the expenditure of £200 would probably, and of £500 certainly, I think, show the relations of the deposits there to each other, and clear up the question of the glacial or post-glacial age of the beds containing the relics of palæolithic man and the great pachyderms. Large sums, and with results exceeding our anticipations, have been spent on the exploration of the cavern deposits, and we have ascertained definitely from them that man and the great extinct mammals lived at the same time. We should now take another step, and determine the exact position that the same fauna holds in the geological series; and this can be done at Hoxne. We send out scientific expeditions to the ends of the world, and rightly so I think, and yet here is one of the grandest problems that can interest mankind lying at our doors, and lying neglected. Granted that I may be mistaken, and that Prof. Prestwich—whose geological opinion is properly of much greater weight than mine—may be right; is it not worth while to set the question at rest, and not consume our time in fruitless discussions and barren congresses? My glacial theory is the outcome of many years of study of the phenomena with which it deals, and I know that it has been fashioned with sincerity; but it is not so dear to me that I should hesitate to put my own shoulder to topple over the edifice I have reared if I could find reason to believe that it was not founded on truth. If the explorations that I urge, ought to be undertaken at Hoxne, be carried out, and prove that the implement-bearing beds are post-glacial, I shall at least have the satisfaction of thinking that not only has my own geological vision been cleared, but that Mr. Prestwich—whose writings for more than twenty years have been my study and delight—has been proved to be right. But trivial and paltry are these personal considerations compared with the issues that are undetermined, and which it is our duty and privilege to clear up, when we have at Hoxne such an opportunity of doing so as is not known to exist anywhere else in Europe.

V. A NEW THEORY OF TRANCE.

THE subject of Trance has for centuries excited great wonder and curiosity, and although the essays and works on its nature and causes may be numbered by hundreds there are very few writers who have treated the subject fully and scientifically. It was therefore with great satisfaction that we received from Dr. G. M. Beard a paper on the subject, which he recently read before the New York Medico-Legal Society.* Our readers have already had an opportunity of judging of the care and originality with which Dr. Beard works out his ideas, from his article on "The Longevity of Brain Workers," which appeared in the "Quarterly Journal of Science" for October, 1875; and in this paper on Trance there is the same evidence of deep thought and mature consideration.

With regard to the value of human testimony our own views do not, in some points, coincide with those expressed by Dr. Beard; but from the exhaustive way in which the whole subject is treated we believe we are consulting the interests of our readers in placing before them an explanation of his new theory, and of giving in a somewhat abbreviated form the principal arguments which he adduces in its favour.

Under the word "Trance" Dr. Beard includes the real phenomena represented or suggested by terms which are more or less meaningless and incorrect—such as somnambulism, artificial and spontaneous mesmerism, animal magnetism, hypnotism, Braidism, catalepsy, ecstacy, biology, &c. He very rightly insists on the absolute necessity of having recourse to deductive as well as inductive reasoning when grappling with such subjects as trance, reasoning from generals to particulars, and drawing conclusions from principles already established, as well as from particulars to generals; and in opposition to the oft-repeated assertion that it is the wiser course for scientific men to let these subjects alone, and not to attempt their solution, he remarks—what indeed we have ourselves often urged—that such sciences as Astronomy, Physics, Physiology, and Chemistry were in their infancy opposed on substantially the same ground. These things, it was claimed, could not be understood by the human mind; therefore we were counselled to join ourselves to the idols of our ignorance, and let them alone. But how far, we would ask, should we have advanced in civilisation had such advice been followed? We believe that by adopting true scientific method in such investigations as the one now under consideration the mysteries of Trance and the phenomena ascribed to Spiritualism will ultimately be unravelled. Let us see how far Dr. Beard's new theory contributes to this much-to-be-desired end.

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* "A New Theory of Trance, and its Bearings on Human Testimony." By GEORGE M. BEARD, A.M., M.D.

The nature of trance is, he contends, a functional disease of the nervous system, in which the cerebral activity is concentrated in some limited region of the brain, with suspension of the activity of the rest of the brain, and consequent loss of volition. It is the prime requisite of a scientific hypothesis that it should account for all the phenomena embraced under the department to which it applies. The hypothesis that trance is a morbid state, consisting in a concentration of the cerebral force in some limited region of the brain, the activity of other portions being meanwhile suspended, seems to account for all the real phenomena of this state, all its different forms and stages.

Trance, like other functional nervous diseases, may be induced either physically or psychically.

Among the physical causes are injuries of the brain, the exhaustion of protracted disease or of starvation, or of over exertion, anæsthetics, alcohol and many drugs, and certain cerebral diseases. Ordinary sleep may act as an exciting cause, as is illustrated in the somnambulistic form of trance. Under the psychical causes are included all conceivable influences whatsoever, that may powerfully excite any emotion or group of emotions.

For the sake of convenience of description Dr. Beard divides trance into four varieties—the *spontaneous*, the *self-induced*, the *emotional*, and the *intellectual* trance. In strictness these varieties may to a certain extent include each other, and in using these terms this fact should be borne in mind. Thus, the intellectual trance is spontaneous, although the majority of the cases of spontaneous trance are not also intellectual. The self-induced trance may be partly emotional, but it is not entirely so.

A typical form of spontaneous trance is natural somnambulism, or sleep-walking—a term which is vaguely used by many writers to include all phases of trance, excepting those which are produced by performances of mesmerisers, which are called cases of artificial somnambulism. In sleep-walking the cerebral activity, which during ordinary sleep is more or less lowered throughout the brain, is suddenly concentrated in some limited region; the cerebral equilibrium being spontaneously disturbed through the subjective action of dreams, the subject, under the dominion of this restricted region of the brain (the activity of the rest of the brain being suspended), runs or walks about like an automaton, with exaltation of the sense of touch often, and of the co-ordinating power, as is shown in their capacity for balancing in difficult and dangerous positions, and climbing on heights where in the normal state he would not venture. Other senses may be sealed entirely, as in other forms of trance.

Under self-induced trance are comprised those cases where the subject can bring himself into this state at will, either suddenly or gradually. Of such subjects it may be said that they will to lose their wills; or it would be nearer the truth to say that they voluntarily put themselves under influences where the involuntary life becomes supreme.

All genuine trance preachers and speakers—and many of them are genuine—represent the self-induced variety. Dr. Beard states that he has studied the case of a famous trance preacher, who told him that when he began to go into this state the first symptom was only a thrill, or electric shock through his arm; then with more practice the whole arm became convulsed, then the whole body, until in time exaltation of the faculties of imagination and of language were developed, and he became a most successful performer before audiences.

In speaking of the terms often applied in spiritualistic circles to mediums as being "fully developed," or "partially developed," or as "developing," Dr. Beard concedes that there is a basis of truth in such expressions, inasmuch as it oftentimes needs practice to acquire the habit of readily, and at will, entering the trance. Further, that some have a habit of falling into trance spontaneously at regular intervals, the same periodicity oftentimes occurring in this disease that has long been observed in neuralgia.

Under emotional trance are included cases that are caused by the so-called mesmeric performances, or through the feelings of fear, wonder, reverence, and expectation, however excited. The majority of the cases of trance come under this head; for every one is endowed with these emotions, and in the greater part of the human race they are the controlling elements in character, and in the strongest and most intellectual minds they are apt to be in constant and usually successful rebellion against the authority of reason: any influences, therefore, that excite any or all of these emotions may be regarded as exciting causes of trance.

The almost universally held belief that the mesmeric form of emotional trance is caused by some force or fluid (animal magnetism) passing from the body of the operator into the body of the subject, is, according to Dr. Beard, as far from the truth as any view on any subject possibly can be; it makes no difference what is done to produce mesmeric trance; it makes no difference who does it; it is a *subjective* matter entirely, and all depends upon the emotions of the subject—what he fears, expects, or wonders at.

To intellectual trance belong the extreme cases of what are commonly characterised as absent-mindedness—a state which is quite distinct from simple mental attention. The popular term absent-minded, as applied to those who become so absorbed in thought that they are unconscious of what is going on around them, and perhaps respond automatically to external suggestions or influences, is, in view of the theory of trance advocated by Dr. Beard, a happy one, since it expresses with partial correctness the real state of the brain during an attack of that kind. A large portion of the brain is active, and, until aroused, is insensible to surroundings, and thus responds mechanically. The biographies of illustrious thinkers are filled with instances, some of which are probably correct, of this form of trance. Thus Walter Bagehot says of Adam Smith, the great political economist, that his absence of mind was amazing. On one occasion, having to sign his name to an official document, he produced not his own signature, but an elaborate imitation of the signature of the person who signed before him: on another occasion, a sentinel on duty having saluted him in military fashion, he astounded and offended the man by acknowledging it with a copy of the same gestures.

In these intellectual trances, great thoughts have been, without doubt, evolved, that would have been impossible to the brain in its normal state.

Dr. Beard then proceeds to show how by his hypothesis all the facts of all forms and phases of trance are explained, unified, and made harmonious; it is only by this hypothesis, he affirms, that it is possible to give any unity or solidarity to the phenomena of this state.

He argues, First,—That this hypothesis accounts for the loss of the control of the will and the automatism of trance, which is the first observed and most distinguishing feature. That, in his normal state, man is, to say the least, nine-tenths a machine. The involuntary life—that which acts without the will, or in spite of the will—is the chief fact in human life and in human history. Comparing life to a wheel, as Dr. S. S. Laws has also done, the voluntary functions may represent the narrow hub, while the involuntary functions are represented by all the area between the hub and the periphery. In this little inner circle lies all human responsibility and all the vast influence of punishment or reward; in all the rest of his functions man is as much an automaton as a tree or a flower. Now, in the trance, this little inner circle of what is called volition is encroached upon by the involuntary life, and in the deeper stages is entirely displaced by it. The fully entranced person has no will; what he wishes to do he cannot do; what he wishes not to do he does; he is at the mercy of any external or internal suggestions.

This hypothesis of the concentration of the cerebral activity in a limited region accounts for the displacement of the will in this way:—The will may be defined as the co-ordinated activity of all the faculties of the mind, including, in general terms, the perception, the emotions, and the intellect. Cerebro-physiologists will agree—all questions of phrenology, or cranioscopy, or minute specialisations of functions aside—that the brain does not act as a unit, but that different parts are the organs of different faculties. When the cerebral activity is harmoniously diffused, as in the normal state, through all the different regions, the man is said to be under the control of the will. When the cerebral activity is concentrated in some limited region of the brain—say that devoted to the emotions, or that devoted to the intellect, the activity of the rest of the brain being suspended for the time—the man would have no will; he would be under the control of that group of faculties; he would be a conscious living automaton, as a fully-entranced person always is.

Secondly,—That his hypothesis proves why trance is an abnormal state. It shows that it must be a morbid pathological condition, and also shows in what this morbidness consists. The man whose mental faculties are mostly suspended, who has no will, but is under the control of some single faculty, is surely in an abnormal state, and in this respect the popular idea is correct. It is a functional disturbance relating only to circulation and innervation, and not causing structural changes, although it may be caused by structural cerebral diseases, and not ordinarily permanently affecting the health in other respects. The liability to trance, like the liability to various other functional disturbances of the nervous system, does not conflict with general good health and longevity.

Thirdly,—That this hypothesis explains the difference between trance and ordinary sleep, which in some respects it so much resembles. Sleep is a normal state, a partial cessation of the activity of all the faculties, a lowering of the activity in all the regions, but not a suspension of the activity of any except the will, which, as we have seen, is simply a co-ordinated action of the faculties. When a person who is sleeping gets up and walks in his sleep—in other words, passes into the somnambulistic form of trance—the change that takes place in the brain is this; while sleeping the activity of all the faculties was lowered; on going into trance the activity of all the faculties becomes suspended, and the entire cerebral activity is concentrated in some one faculty or limited group of faculties.

Fourthly,—That this hypothesis explains the phenomenon of dual life and double consciousness, which has been regarded as one of the greatest and most inexplicable mysteries of trance.

In trance—even in the most profound instances ever observed—there is probably always consciousness at the time, but it is not always or usually remembered consciousness. On awaking, as on awaking from ordinary sleep, the dreams, that may have been active and numberless, fade as a cloud; possibly not even a glimpse of them may be caught and held before the mind long enough to become a permanent and recollectible impression during the normal state. But on resuming the trance state the exalted functional activity of the region of the brain in which the cerebral force is concentrated is able to bring these impressions of the previous attack of trance, forgotten during the intervening normal state, to consciousness, and thus the subject carries on an independent trance life, just as though there had been no intervening normal state. On returning to the normal state, the cerebral force being again diffused through the whole brain, is insufficient to enable the subject to recal the experience of the trance, but quite sufficient to enable him to recal the experiences of his previous normal state. Thus he leads two lives, the normal life and the trance life, and they are independent of each other.

In cases of dual life the trance life is the more brilliant and active in certain features, as by this hypothesis it naturally would be. In a case under the observation of the late Dr. J. R. Mitchell, a young girl in trance life was quick, energetic, and witty, and vivacious; in her normal life she was slow, indolent, and querulous.

Fifthly,—That the hypothesis explains the difference between the deeper stages of trance, and death, with which trance is sometimes confounded.

With this hypothesis of the pathology of trance before my mind, I have, continues Dr. Beard, been accustomed to illustrate the difference between ordinary sleep trance and death, by pointing to a chandelier of gas-burners. When all the burners of the chandelier are fully lighted, that is the normal waking state; when all the burners are turned down low, but not turned out entirely, as usually is the case in public halls before the opening of entertainments, that is ordinary sleep; if I turn out entirely all the burners except one, and that one, as often happens, flames all the more brightly from increased pressure, that is trance; if all the burners are turned out entirely and permanently, that is death. The only hold on life which the deeply entranced person has is through the activity of a limited region of the brain, through which feeble movements of the heart are sustained, the body being in other respects motionless. The popular belief that deeply entranced persons are liable to be buried alive is correct, but fortunately mistakes of this kind occur but rarely.

Sixthly,—That this hypothesis explains the exaltation of some of the physical and mental faculties in trance, and depression of others.

The exaltation of the physical and psychical faculties in trance cannot be questioned, but is readily demonstrated, and by this hypothesis receives an explanation that is both lucid and complete.

Representing, for the sake of comparison, the quantity of cerebral force in all parts of the brain by one hundred—if the activity of three-fourths of the brain is suspended, then the remaining one-fourth may be fourfold more active than when in the normal state. That there should be such a concentration of cerebral force in a limited range of faculties is in harmony with every day observed facts. Thus the intellect increases in vigour in any direction under exercise up to a certain point, and through over exercise becomes fatigued. In the brain are the centres of thought, of muscular motion, and of general and special sense. It would follow, therefore, that some one or several of the senses, or some one or several of the mental faculties, or some one or several groups of muscles, might be exalted in activity, with entire suspension of the activity of other senses, faculties, and muscles, according to the region of the brain in which the concentration of activity takes place. There is therefore no mystery in the frequently observed, though sometimes disputed, fact that entranced subjects can raise with ease weights which in their normal state they are unable to move. Mesmerised subjects sometimes exhibit this power. Persons entranced through fear, as by an alarm of fire, have been known to take up a stove and carry it out of the house; the next day they cannot, to save their lives, carry back that stove. The co-ordinating or balancing power may be so much exalted in somnambulists that they can climb without harm in most dangerous places. The exaltation of the time-telling power—which sometimes passes for second sight—has the same explanation as the exaltation of other faculties. Likewise the nerves of general and special sense are all liable to be greatly exalted in this state; the feeblest whisper in a distant room may be readily heard, and one can read by a dimmer light than is usually needed. The sense of touch may be so delicate that, when the sense of sight is sealed, the subject can find his way from room to room without injury; and it is claimed may, in some cases, recognise the presence of another person near at hand, by the temperature alone, even where there is no physical contact.

We are told these exaltations of the normal senses are the bases of many of the popular and professional delusions relating to "second sight," "clairvoyance," "thought reading," and the like. By this hypothesis, also, any of the mental faculties should be liable to be exalted. Observation shows that not only the imagination, but the reasoning faculty and command of language are oftentimes greatly enlivened in their activity in this state, as the performances of trance preachers illustrate. Weak-minded men and women, who in the normal state think little and say less, are sometimes able, when entranced, to speak continuously, and almost if not quite eloquently, and with slight apparent effort. While there has been much exaggeration of the originality and value of these trance speeches, yet it cannot be denied that they are—with all their wildness of fancy and repetition, and frequent senselessness—far beyond the capacity of the same persons when not entranced. On returning to the normal state they may be utterly stupid or commonplace; their cerebral force, when diffused through the whole brain, is unequal to even rapid and sustained small talk.

The converse of exaltation, depression of some of the senses and faculties of the mind, directly follows from this hypothesis of concentration of cerebral activity; those senses and faculties that belong to the entirely inactive regions of the brain must be for the time practically dead, as is found to be the case in some forms of trance. Thus the sealing of some of the special senses, and general anæsthesia, making it possible to perform without causing pain certain surgical operations, are accounted for.

Seventhly,—That this hypothesis explains all the familiar physical symptoms of trance, such as flushing of the face, fixity of position, sighing respiration, accelerated pulse, involuntary convulsive movements, and marvellous and numberless hysteroid sensations.

The effects of trance on the pulse and respiration, and on the circulation in general, are, according to Dr. Beard, what would be expected from the known inter-dependence of mind and body. The quite recently established fact of the existence of definite centres of muscular motion in the brain, however the fact may be interpreted, is of great significance in its bearings on this subject, since it shows clearly why convulsions so frequently accompany trance. The aphorism that when we think we move was based on our knowledge of the existence of these centres of muscular motion in that portion of the surface of the brain that is regarded as the seat of some of the mental faculties, and was first suggested to him while repeating the experiments of Hitzig and Ferrier in the electrical irritation of the brains of animals.

Eighthly,—That this hypothesis accounts for the illusions and hallucinations of trance.

Illusions, delusions, and hallucinations are, as is established, the products of cerebral activity, and are frequently the symptoms of some abnormal state of the brain. There is no proof that any other part of the body than the brain, as the spinal cord or nerves, can originate hallucinations, any more than there is proof that any other part of the body can originate the higher modes of conscious thought—all the facts and arguments that serve to establish that the brain is the organ of the mind in health, also establish that it is the organ of the mind in disease. For while automatic acts, as nursing and so forth, may be manifested by brainless infants, and while the spinal cord clearly contains centres of reflex action, yet there is no proof that any conscious thought, of the *higher* kind at least, attends the activity of these reflex centres, any more than in the familiar automatic movements of plants. The hallucinations of trance—the visions of heaven and other innumerable fancies—must then, like dreams and all mental operations, whether coherent or incoherent, have their seat in some part of the brain; and, according to this hypothesis, their existence, their coherency, and their extreme activity, are all explained.

Ninthly,—That this hypothesis accounts for the relation of trance to its admitted predisposing and exciting causes.

By this hypothesis any influence that tends to overthrow the cerebral equilibrium, to disturb the balance of innervation, would be likely to be a cause of trance: experience shows that this is actually the case.

The predisposing, like the exciting, causes of trance are both physical and psychical.

One is physically predisposed to trance, says our author, who inherits or has acquired a nervous system generally sensitive and impressible. One is psychically predisposed to trance who is mentally unbalanced through excessive and disproportionate endowment of imagination and emotion. One who is powerfully developed in reasoning and thinking qualities, and is badly deficient in observing, practical faculties is so far forth predisposed to the intellectual form of trance. The best subjects are those who are predisposed both physically and psychically, who have sensitive organisations, and unbalanced, ill-trained minds.

Trance is not, however, as many suppose, the peculiar gift of certain temperaments. It is the property of the human race. All persons are liable to become entranced, just as they are liable to become paralysed or epileptic, although all do not suffer in this way. All persons are not predisposed to the same form of this disorder; one can only be entranced through the intellect, another through the emotions; one person can only be frightened into this state; one needs the presence of a medium, another of a mind reader, another of a clairvoyant, and another of a mesmeriser; another of a magnetised letter, and another still of one who performs miracles of healing by the laying on of hands. Mr. Grimes, who has had much experience with the mesmeric trance, and who is accustomed to direct his subjects to stand still with closed eyes and folded hands, as a means of exciting the emotion of reverence, says that he failed with every one out of forty military officers at West Point, while just across the river, among the operatives, the same process was very successful.* This is easy to understand from what has already been stated in regard to the predisposing causes, but it would be an error to infer that those officers were not capable of being entranced. If they should all sit in a circle around a table for half an hour or more, with the expectation that some strange things would develop, very likely some of them would become carried away, and, by unconscious muscular motion, would move the table; or perhaps they would feel sensations like electrical shocks through their bodies, or they might go into convulsions, or might experience wonderful visions, hearing the voices and seeing the faces of loved ones.

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* Mr. Grimes has recently published a work entitled "Mysteries of the Head and Heart," the latter portion of which especially is commended to the attention of those who are interested in these themes. Mr. Grimes is, says Dr. Beard, almost the only writer on trance who has had sufficient originality and

Tenthly,—That this hypothesis accounts for the periodicity of trance in certain cases.

It is the nature of all functional nervous diseases—neuralgia, sick headache, hay fever, inebriety, and some forms of insanity—to appear more or less periodically. It may be said that the majority of cases of spontaneous trance are periodic.

It may be opposed to all this process of reasoning that no one has ever seen with his eyes the brain thus concentrating its force during an attack of trance; but it must be remembered that only exceptionally can scientific hypotheses be verified by actual sight. Even in the material world the seen is but a fraction of the unseen. No man ever saw the waves of light; no man has ever seen gravity: these universal forces are studied only through their phenomena, by means of which we frame hypotheses of the law of gravitation and the existence of a luminiferous ether. In the realm of physiology and pathology the chances of verification by actual sense perception are more rare than in astronomy or physics.

We have now laid before our readers what we hope is a clear and concise account of Dr. Beard's theory of trance, and of the principal arguments which he adduces in its favour. In his paper he enters at very considerable length into the necessity, in such investigations as of spiritualism, of reasoning by the deductive method, &c. He moreover insists that it is only those who have made themselves masters of, and authorities in, cerebro-physiology and pathology, and who have thereby learned that the subject lies within the

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mental force to see more than one side of it. But, like Dr. Carpenter and most other writers on these subjects, he fails to see all sides, and he makes the mistake which is fatal to the scientific study of trance, of conceding the possibility of thought reading. This serious mistake results from the studying the subject inductively instead of deductively; it is indeed, as I shall show farther on, an inevitable mistake from that false method of reasoning.

grasp of the properly trained and properly furnished intellect, who are competent to unsolve such problems and to explain the mysteries in full detail.

Up to a certain point, we agree with Dr. Beard, but we maintain that the phenomena he discusses embrace two distinct branches of science, viz., Physiology and Physics, and that it is strictly within the province of the trained physicist, and outside the province of the physiologist, to apply such tests as the electrical and other tests which we ourselves have used when investigating the physical branch of the subject. There is abundant proof that the fact of being a specially trained physiologist may actually disqualify a man for investigating spiritualism. For instance, Dr. Carpenter is a physiologist, and it is chiefly to his researches in physiology and pathology that he owes his present position. He ought, therefore, to be an expert, and to be able to get at the truth by exactness. He certainly considers himself fully competent to conduct such an investigation, for here is his own estimate of his qualifications. He says*—"The training I originally received, and the theoretical and experimental studies of forty years, have given me what I honestly believe (whether rightly or wrongly) to be a rather unusual power of dealing with this subject. Since the appearance of my Lectures I have received a large number of public assurances that they are doing good service in preventing the spread of a noxious *mental* epidemic in this country; and I have been privately informed of several instances in which persons who had been 'bitten' by this malady have owed their recovery to my treatment."

Now, Dr. Carpenter has been investigating the subject of mesmerism, spiritualism, &c., for more than thirty years, and what has he done towards solving the problem? Absolutely nothing! The reason is, we believe, that his mind lacks that acute philosophic quality which would fit him to unravel the intricate problems which lie hid in the structure of the human brain. He has, moreover, shown himself unable to grapple with the reasons or to deal with the facts of opponents; he views their work through a special "focus of his own," and is incapable of projecting himself into the mind of another person. We wish to do justice to Dr. Carpenter's ability and usefulness. It is, indeed, whispered that he has rather talked than worked himself into position and reputation, and like the clever man in the market place, whose cries of "I'm a genius! I'm a genius!" drew a crowd round him, so Dr. Carpenter, by reiterated assurance that he alone as a physiologist possesses, in a supreme degree, a "trained and organised common sense," has led the public to yield to his views a certain degree of assent. But we would not for one moment underrate Dr. Carpenter's abilities: he has undoubtedly done a considerable amount of meritorious work; and if he does not hold any high rank as an original investigator, he distinctly occupies a noteworthy scientific position as an expositor and popular lecturer. He is, in fact, the indispensable middle-man between the original investigator and the public. He has compiled some useful books on physiology and on the microscope, and were he not in the habit of viewing unwelcome facts pseudoscopically, and describing them catachrestically, he might be said to hold, with regard to this generation, the same position that Dr. Lardner held in the last, though Dr. Carpenter lacks the bright, clear, unfatiguing style of his prototype.

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* Nature, vol. xvii., p. 26.

But in his investigation of the phenomena ascribed to spiritualism, Dr. Carpenter has stepped out of his proper sphere. Physiologists are not authorities on the physical side of the question, and it is a pity that when dealing with the subject from a physiological point of view they invariably either ignore the physical tests altogether or they misrepresent them, for the purpose, apparently, of securing a little cheap applause by showing that the experimentalist is a dishonest or untrustworthy observer.

NOTICES OF BOOKS.

The Border Lands of Geology and History: an Inaugural Address. By T. W. KINGSMILL. Delivered at Shanghai on the 20th of February, 1877. Shanghai: Kelly and Walsh. London: Trübner and Co.

WHILST the facts generally summed up under the name of "glaciation" are admitted by every one fairly competent to form an opinion, there is no such happy accord concerning the cause assigned to these phenomena—the so-called "Glacial epoch." What were its causes? Did it occur once only, or repeatedly? Was the whole globe simultaneously attacked, or were the northern and southern hemispheres glaciated alternately? Was there an "ice-cap" extending regularly and uninterruptedly down from the poles to comparatively low latitudes, and obliterating every natural feature? Or did the glaciers, as in our day, though on an intensified scale, take an independent origin in the mountain chains, and move sometimes not from, but towards, the poles? On all these main questions, and on many collateral points, geologists of the highest merit disagree, and a controversy has been for some years going on. This discussion may be pronounced most fruitful, since it necessitates a more minute and searching appeal to facts than would otherwise have been undertaken. Some of the results of this appeal are very remarkable. The conclusion that in Europe and North America glaciation has prevailed is not in the least shaken, but rather confirmed. The evidence on which the late Agassiz inferred that even equatorial Brazil once suffered from this invasion of ice stands, indeed, in need of corroboration. But the proofs of glaciation found by Mr. Belt in Central America, and even at comparatively low altitudes above the sea-level, leave exceedingly little room for doubt. The question, however, naturally suggests itself—What about Asia? If the northern hemisphere has at any time been glaciated as a whole—whether simultaneously with the southern hemisphere or independently is a matter of no importance—North China, Mongolia, and Mantchuria ought to show the usual traces of the visitation. "Yet in no part of Eastern Asia do we hear of phenomena indicating a Glacial period." Mr. Belt, than whom no more competent observer could be named, has examined the regions between Ekaterinburg and the head-waters of the Irtish, lat. 51° to 55° N. and long. 60° to 76° E., "a situation which, according to usually-received glacial theories, should have been extensively glaciated." But from his description the author sees no proof that such was ever the case. "The sands and loams of the steppes, though covering extensive areas, in no place seem to exhibit the ordinary marks of glacial action. In the north they are composed of fine sediment, without a trace of pebbles; going south the pebbles increase in size and numbers; and along the southern boundary of the deposit abutting on the mountain chains they contain angular blocks of stone, growing larger as the foot of the ranges is approached. The rocks lying immediately south of these beds, according to Mr. Bates, show also no signs of glacial action.

To account for this absence of glaciation in North-Eastern Asia the author suggests a secular shifting of the axis of the earth's rotation, by which, of course, different regions would in turn become circumpolar, and experience the conditions now prevailing in Spitzbergen, New Siberia, North Greenland, &c.

"It has, however," he declares, "in my judgment not been proved that these glacial phenomena are contemporaneous, and herein lies what I conceive to be the fallacy of the reasoning of the glacialists. Perceiving in many portions of the earth's surface traces of a state of things only to be attributed to an access of intense cold, they have fallen into the error of classing them together, and thereby evoking the so-called Glacial epoch. Glacial epochs, I believe, have been numerous: nay, I will go further, and say that since the beginning of geologic time some portion of the earth's surface has ever been undergoing its own glacial stage. Sometimes a greater access of cold occurred than at others, and this period of greatest cold had doubtless a tendency to oscillate from one hemisphere to another, but I think that the evidence afforded by the geology of Northern Asia is too strong to permit of the belief in any universal Glacial epoch being acceptable.

"Traces of a Glacial epoch have been clearly proved to exist so far back as the time of the deposit of the Permian rocks. The valley of the Godavery, in India, seems then to have been within the glaciated area. Nearly, if not quite, contemporaneous with this are glacial deposits in Natal. But is this a proof of a general Glacial epoch? I trow not. The observed facts are very similar to what I have pointed out as occurring in Europe and Northern Asia in late tertiary times. The former was glaciated; the latter enjoyed a milder climate than at present. So while India and Southern Africa were having their Glacial epoch a diligent search amongst contemporaneous rocks in Australia fails to lend any countenance to the view that it extended to those regions."

This hypothesis, whether it be ultimately found to accord with facts or not, may seem at first sight to have little connection with the title which the author has selected. But if various portions of the earth's surface are being successively glaciated owing to a secular displacement of the poles, we may expect to find in some regions—those, to wit, from which the pole is receding—a gradual improvement of climate, whilst in others, to which the pole is approaching, there will be a correspondingly progressive deterioration. Upon such changes not merely geology, but myth, tradition, and finally authentic history, may contribute to throw important lights. From these various sources the author concludes that the climate of Northern and Eastern Asia is becoming colder, a view in which Mr. Geikie seems to concur. The proofs of this increasing refrigeration are to be found "in the comparison between its present and its recently-extinct fauna, as well as in the geographical distribution of the animals that remain." But ethnology contributes evidence of a similar tendency, which will be perhaps more widely understood. Central, Northern, and North-Eastern Asia are now but thinly peopled, and are unanimously pronounced by travellers as very ill adapted for the support of a dense population; yet history and tradition represent these now lone and silent deserts as being the *officina gentium*, a very magazine of nations, from which horde after horde issued to ravage the fertile lands of Southern and Western Asia and of Europe. Historians have recorded these successive migrations, which, commencing with the attack of the Hiung-nu upon China, some six or seven centuries before our era, culminated in the overthrow of the Roman Empire, and lasted down to comparatively modern times. But the cause which set these tribes in motion from their original seats has been overlooked. According to our author it was the commencement of that process of refrigeration and desiccation which has evidently prevailed for a long time in Central Asia. He contends that at a still earlier period a vast mediterranean stretched from the Black Sea eastwards towards the Pacific, cutting off the southern parts of Asia from the North. Of this sea the Caspian and Lakes Aral and Baikal are now the principal remains. By a gradual upheaval this sea was drained, except the portions just mentioned. Its former bottom is characterised by the "loess" formation, more largely developed in East Asia than in any other part of the world, and which strongly resembles the ooze dredged up by the Staff of the *Challenger* Expedition from depths of about 2000 fathoms. To counterbalance this great upheaval of so extensive a portion of the earth's crust, the author considers that the continent of "Lemuria," formerly extending across the Indian Ocean, was simultaneously depressed. A remarkable argument for the former existence of a Central Asian Sea is founded on the presence of seals in the Caspian, in Lake Aral (till very recently), and in Lake Baikal. The latter case is the most important, since in no other part of the world do seals inhabit fresh water. Now if we suppose that these lakes were formerly parts of an inland sea, it is very conceivable that seals might be prevented by the upheaval of the land from making their escape to the ocean, and might in successive generations become habituated to a gradually freshening water.

It is obvious that Mr. Kingsmill's views can only be thoroughly criticised in the deserts and the mountains of Eastern and Central Asia, hammer in hand, and there are many points which must be more closely examined before his hypothesis can be either accepted or rejected. From the point of view of the physicist there is, we believe, no preliminary objection to the idea of a secular shifting of the earth's axis of rotation. Sir W. Thomson considers it as possible that the poles may have, at one or other time, have occupied positions differing from their present locality by as much as 40°. Such a variation might place London alternately on the Pole and within about 10° of the Equator—a climatic range fully sufficient to account for the different faunas and floras of bye-gone geological epochs. But is there at present any indication that the axis of the earth is shifting its position? If the North Pole is receding from us and approaching the eastern parts of Siberia, our longest day ought to be growing gradually shorter and our shortest day longer, and the maximum apparent altitude of the sun in the heavens ought to be increasing. But we are not aware that even any suspicion of such changes has been aroused. If, therefore, a secular displacement of the earth's axis of revolution is in progress, it must be exceedingly slow—too slow, we think, to have effected any important climatic changes in Central and Eastern Asia within the limits of historical time.

Again, for the deterioration which those regions have admittedly undergone, the author proposes two causes apparently not standing in any necessary connection—viz., the displacement of the earth's axis and the elevation of the land in the centre of Asia. Is it not possible that the latter cause may alone suffice to account for the effects produced? A gradual drying up of rivers and a paucity of rainfall have also been traced in many parts of Central and Southern Europe and of Northern Africa, but there has been no distinct amelioration of climate beyond what is due to improved drainage and other local causes. Yet on Mr. Kingsmill's hypothesis Western Europe and Eastern North America ought to be enjoying a progressive elevation of their average temperature.

If the earth's axis is undergoing secular displacement this change will doubtless follow some law, and the poles would infallibly describe a regular curve. This curve it will be important to trace out as nearly as possible by observing in what order of time different parts of the world have undergone glaciation. Now it is quite possible, supposing Mr. Kingsmill's hypothesis to be correct, that Eastern Asia should exhibit no marks of a glaciation synchronous with that of Europe and North America. But the theory seems to us to require imperatively that Asia should exhibit marks of an earlier glaciation and to detect these is an essential step towards the verification of Mr. Kingsmill's supposition. Another important test will be a re-examination of the leading facts of animal and vegetable geography in the light of this new hypothesis. If it harmonises with the distribution of organic species its probability will be greatly augmented.

It would be unpardonable to conclude this hasty survey of a brief but most important treatise without putting on record our appreciation of its character. The author dissents, indeed, from certain geological theories widely—if not universally—received; but he thinks and writes as a man of Science, and he may therefore justly demand for his views a full and a candid examination. The advice which he gives, in concluding, to the members of the "North China Branch of the Royal Asiatic Society," is most excellent. "In the investigation of the loess," he says, "a wide field is open for research, and one in which many members of this Society can render a service. We are entirely ignorant of its microscopic composition, yet without a knowledge of this we can do no more than speculate as to the conditions under which it was deposited. What Dr. O. von Mollendorff has done for Chihli (Petcheler) is wanted for all the other provinces of China—a reliable catalogue of the animals resident within their limits. Of the botany of North China we are supremely ignorant, and we have still a very imperfect knowledge of its fishes. The interesting fossil flora of the Kinsin coal-field is still untouched, yet it is of special importance in its connection with the Arctic flora of similar age."

Proceedings of the Literary and Philosophical Society of Liverpool during the Sixty-fifth Session, 1875-76. No. 30. London: Longmans and Co. Liverpool: D. Marples and Co., Lim.

THIS volume possesses the character common to the Proceedings of most of our provincial learned societies: its contents are literary rather than scientific, and the papers of the latter class are devoted rather to popular exposition than to the results of research. This, of course, is to be expected: we reap just what we sow. As long as our education is based upon literature, as long as the art of expression is prized more highly than the power of origination, so long we shall abound in novels, sermons, speeches delivered in Parliament and elsewhere, historical, antiquarian, and critical dissertations, and be scanty in scientific work, in discoveries, and inventions. Whether we are acting wisely or foolishly it is now not the time to inquire.

The first paper in the volume before us is the Presidential Address on "The Tendencies and the Future of Modern

Civilisation." That it contains many interesting and suggestive passages cannot be denied ; but it enters too largely into political and theological considerations to admit of analysis in these pages. We regret to find the author quoting Guizot, a writer whose "Histoire de la Civilisation" always reminds us of the old "conchological" systems—founded on an exclusive consideration of the shell, with complete neglect or ignorance of the vitals.

Dr. J. Campbell Brown's paper, on "Electricity compared with Heat as a Source of Mechanical Power," will, we hope, go far to dispel certain semi-scientific delusions current as to the probability of our finding some substitute for coal. The conclusion of the memoir deserves quotation :—"There neither is nor will be any substitute for coal if in that term we include petroleum, oils, peat, and wood,—in short, carbon and hydrocarbons. Force cannot be created ; it must be obtained from previously existing stores of force, and when it has been equally distributed there is no collecting it again. We must economise our carbon, and when it becomes exhausted we may utilise the force of the sun, growing wood by its light, producing evaporation or obtaining mechanical power in the shape of wind from its heat, and obtaining mechanical motion from its attraction and that of the moon by means of the tides ; but in the earth itself, apart from other worlds, we can expect to find no store of force, either in the form of chemical force, heat, light, or electricity, which will take the place of our carbon deposits. We may learn to do without coal, but we can hope to find no substitute." In all this, however, we see no justification for any increase of the enormous profits of the mine-owner and the coal-merchant, nor yet for any augmentation of the working colliers' allowance of champagne and boiled pine-apples.

Mr. T. Ward communicates an interesting statistical paper on "Salt and its Export from the Ports of the Mersey." The method of preparing the various commercial qualities of salt, as carried on in the Cheshire district, and the quantity exported to various parts of the world, are here given in detail. That it should be carried to India—where, in addition to the facilities for preparing salt from sea-water by solar heat, there are in the north-western deposits beds of rock-salt far surpassing in extent those of Cheshire—seems almost mysterious. But ships going out to India take a cargo of salt at mere nominal freight in preference to going out empty. The duty of £5 per ton, maintained purely as a source of revenue, is economically indefensible, and must ultimately be abandoned.

Mr. E. Nicholson's paper on "Indian Snakes" is intended to combat certain vulgar errors concerning the serpent tribe. He states that out of the 260 species of snakes found in India only five are dangerous to human life. Of these "*Ophiophagus elaps* is very rare" (it has, however, been killed in Calcutta); and "another replaces a congener, the two being rarely found together." Thus the formidable snakes would be reduced to three —the cobra, the bungarus, and the daboia. Nineteen out of every twenty accidents are due to the cobra. Some other species are mentioned which, although venomous, "are not dangerous to man." Under "Error XVI.—*There is some general antidote against snake-poison*," the author remarks, truly enough, that "all experiments in this direction have met with utter failure." But he adds—"If successful they would be of little or no practical use, and they keep up an unhealthy excitement, detrimental to the interest of Science." These assertions are to us utterly unintelligible. The discovery of an antidote seems to us certain to be useful; nor do we see how the interests of Science can suffer from any properly conducted experimental research. As to "Error XVII.—*Some animals are either proof against snake-poison or know of vegetable antidotes against it*," it is well known that the ichneumon and the secretary-hawk, in their combats with the "thanatophidia," depend for success not on any immunity from the effects of snake-poison, but on their dexterity in evading the deadly bite. But what of the hedgehog? We do not know that it has ever been inoculated with the venom of the cobra; but, independently of the authority of Dr. Lenz, we can from our own observation testify that it is absolutely unaffected by the poison of the viper, which in Central and Southern Europe proves fatal to about 20 per cent of the human beings bitten. The annual loss of life due to venomous serpents, Mr. Nicholson thinks, "when reduced to sober death-rates is really of trifling amount; it is on the average about one death in 15,000 inhabitants." Now one death per 15,000 inhabitants would amount in London to 200 fatalities yearly, and in the United Kingdom to 2000! Suppose so many persons came yearly in our midst to a violent and untimely death from one cause, should we call the amount "trifling"? Should we not demand that every conceivable means should be used for the diminution of the evil? Mr. Nicholson has certainly done good service in exposing certain grave errors in the natural history of serpents, but his practical conclusions are, in our opinion, no less to be deprecated than the superstitions he is denouncing.

Mr. A. E. Nevins contributes a paper on the "Method of Correcting the Rate of a Marine Chronometer for Changes of Temperature." Not being one of those omniscient sages who are equally versed in horology, in physics, and in biology, we are unfortunately not able to form any opinion as to the value of Mr. Nevins's method.

The Rev. T. P. Kirkman, known as the author of "Philosophy without Assumptions," writes on "The Janal 14-acral 14-edra." We imagine that all except mathematicians will be fully satisfied with his title, without enquiring further.

"Vegetation and Climate," by Mr. Richmond Leigh, is a sketch of botanical geography.

Mr. W. T. Black's "Natural History of the Grey-wing and Red-wing Partridges of South Africa" is a small but useful contribution to ornithological knowledge. The author recommends the "red-wing" for domestication.

Proceedings of the Bristol Naturalists' Society. New Series. Vol. i., Part 3. 1875-6. London: Williams and Norgate. Bristol: Kerslave and Co.

That the activity of the provincial scientific societies has increased of late years in its amount and has improved in its quality is very generally admitted, and must be received as a hopeful sign of the times. The Bristol Naturalists' Society has four sections—the botanical, entomological, geological, and zoological, the latter of which is at present in abeyance. Of the members eleven have contributed papers printed in the Society's Proceedings, a proportion which we should like to see increased. Concerning the state of the Museum there is no information, and the augmentations to the Library appear to consist to a great extent of the Reports and Transactions of various Societies, English and foreign. The papers which appear in the "Proccedings" are—"Geology of the Bristol District," by W. W. Stoddart, F.G.S.; "On Prof. Renvier's Geological Nomenclature," by E. B. Tawney, F.G.S.; "Birds of the Bristol District," by E. Wheeler; "Age of the Cannington Park Limestone," by E. B. Tawney; "Insect Anatomy," by E. H. Fripp, M.D., who likewise communicates three papers on Microscopy; "Notes on Carboniferous Encrinites from Clifton and Lancashire," by J. G. Grenfell, F.G.S.; and "An Account of the Rainfall at Clifton for 1875," by G. F. Burder, M.D.

History of the American Bison (Bison Americanus). By J. A. Allen. Washington: Government Printing-Office.

This valuable monograph derives additional importance from the fact that the American bison—or buffalo, as it is generally called in the United States—is doomed to early extinction. Perhaps, indeed, the American Government may protect a remnant upon some specially reserved plot of land as an interesting reminiscence of the past, just as has been done by the Emperor of Russia in the case of the kindred species, the European bison, or aurochs (*Bison bonasus s. Europæus*). Mr. Allen gives a full description of the American animal, with tables of measurement of the principal parts of the skeleton, and points out the characteristics which distinguish it from the bison of Europe. Entering next upon its geological history, he shows that its appearance in North America is of comparatively recent date, whilst its fossil remains have hitherto been found in no country outside its known geographical range. He concludes that it is "the descendant of *B. latifrons*, modified by existence in the new conditions of soil and climate to which it was driven by the great changes closing the last ice-age."

The geographical distribution of the American bison at the time of the first arrival of European settlers in the Western Hemisphere is next carefully investigated. According to common tradition it inhabited not merely the eastern half of the great valley of the Mississippi and the basin of the Ohio, but extended its range up to the Atlantic coast. But on carefully sifting the evidence there appears good reason for assuming that the buffalo was not found in New England, nor along the coast of the Middle States, during a long period antedating the exploration of the continent by Europeans, or during the period of the formation of the Indian shell-mounds of the Atlantic coast, which contain no traces of the remains of the buffalo." In the upper parts of the Carolinas the former occurrence of the bison is an established historical fact; but here, also, the evidence of their having been found on the coast is very imperfect. Nor do they appear to have been known within the present limits of Florida. Between 1540 and 1720, however, these animals seem to have temporarily extended their range south-eastwards, and to have penetrated into the country immediately bordering on the Gulf of Mexico, known at that time as West Florida.

A great part of the work is devoted to an account of the gradual extirpation of the bison over the vast tract of country which it once occupied, and its restriction within its present narrow limits. It is now found merely in two distinct areas. The more "southern of these is chiefly limited to Western Kansas, a part of the Indian territory, and North-Western Texas. The northern district extends from the sources of the principal southern tributaries of the Yellowstone northward into the British possessions, embracing an area not much greater than the present territory of Montana." Even within these two districts the author estimates that, at their present rate of decrease, they can scarcely outlive the present century.

To the scientific naturalist the most valuable part of this work is the chapter on the domestication and hybridisation of the bison. Certain theorists of the Swainsonian school maintained, on *a priori* grounds, that it must be irreclaimable. But the following facts are fully attested:—1. That the bison is readily susceptible of domestication. 2. That it interbreeds freely with the domestic cow. 3. *That the half-breeds are fertile.* And 4. That they readily amalgamate with the domestic cattle." For the detailed evidence upon which these inferences are founded we must refer to the work itself. This case, joined to that of the *Leporides*,—a confirmatory account of which has lately appeared in so hostile a quarter as "Les Mondes,"—ought, we think, to make a clean and final sweep of the so-called physiological test of species. It can no longer be safely argued that animals which interbreed and produce fertile offspring must be of necessity specifically identical, nor that those which are morphologically distinct must be incapable of producing fruitful descendants. By bringing before the world the facts just referred to Mr. Allen has rendered one of the main positions of the old natural history simply untenable.

The extirpation of the bison—or at least its very great reduction—is a result which must in the long run have inevitably followed upon the cultivation and enclosure of the country. But it has unfortunately been carried out in the most wasteful and reckless manner, and millions of tons of what might have been utilised as human food have been left to rot, or to become the prey of wolves and vultures.

Annual Report of the Board of Regents of the Smithsonian Institution, for the Year 1875. Washington: Government Printing-Office.

Amongst the varied and valuable matter contained in this volume we notice first a series of publications commenced under the title "Bulletin of the National Museum," intended to illustrate the collections in Natural History and Ethnology belonging to the United States. The total number of vertebrate animal species found in the Nearctic realm as given, is, we think, certainly below the truth, as many fishes must still be undescribed. The number of insect species is estimated at 50,000, of which 8000 are Coleopterous. In enumerating the main zoo-geographical provinces of the world, the "palæotropical" is by a curious typographical error transformed into "palæological." As regards the subdivisions of the Nearctic or North-American realm, the author proposes to make in the Eastern region four provinces dependent mainly on the isothermal lines—viz., the Carolinian, the Alleghanian, the Canadian, and the Hudsonian. The Central region is divided merely into two provinces, parted by the 100th parallel of longitude.

Amongst the memoirs forming the bulk of this volume attention may be drawn to one by Alphonse de Candolle, translated for the Smithsonian Institution, on "The Probable Future of the Human Race." The author's views may thus be summarised :—The coming thousand of years will be marked by a great increase of population, a mingling of races, and a prosperity more or less marked. Then will follow a long period of decrease of population and of general decadence. In this decline the increasing rarity of coal and of the useful metals will play a prominent part. These will by man's operations have become comminuted and equally mingled with the superficial strata of the earth, nor does it apppear that there is at work any natural process which will re-aggregate them in masses capable of new exploitation. The Darwinian theory lends very little countenance to the dreams of indefinite improvement cherished by the French philosophers of the last century. Should the struggle for existence become intensified, as there seems every reason to expect, the intellectual progress of the species must perforce slacken, because the pursuits of the inventor and the discoverer, however beneficial to the race at large, are very scantily remunerative to himself. Hence he will fare as would the vine, the wheat-plant, and others of the most precious and beautiful vegetal species, if left without human aid to compete with the brambles and thistles.

A higher development of the nervous system, holds Mr. Spencer, diminishes the increase of population. A time will therefore come when the chief additions to the population will be made by the less intelligent and less provident families. Hence, says M. de Candolle, "their numbers constantly renewed will greatly affect the supposed progression of intelligence."

It was contended by Malthus, by John Stuart Mill, and others, that almost all social evils spring from the fact that population tends to increase more rapidly than the means of sustenance, and that, whether by moral or physical means, the supply of human beings should always be kept below the demand. Now we grant that the Malthusian individual or the Malthusian family has a certain advantage over non-Malthusians ; but unless the principle could be carried into effect all over the world the Malthusian nation must be overwhelmed by its neighbours, whether in war or by the silent—yet equally sure—method of immigration. A nation can only hold its place by unlimited increase, at whatever cost to some of its own members. The future has, for man, no "good time coming."

In a short biographical notice of the late Prof. Agassiz, also taken from the "Transactions of the Genevese Society of Physics and Natural History," we find the following very judicious remarks :—"He excelled in the examination of details and in the comparison of forms. I cannot say that he was equally superior in the principles of natural classification and in theoretic deductions. It may be considered at the least singular that the author of the immense discovery of a parallelism between the successive forms of the embryo of a fish and the successive forms of the class of fishes in general, in geological times, should persistently deny all evolution in the two kingdoms."

In a valuable paper on "Certain Characteristics pertaining to Ancient Man in Michigan," by Mr. Henry Gillman, we meet with the record of a very singular fact:—"About 50 per cent of the humeri are perforated." This is a Simian characteristic which, singularly enough, is found to pertain in the largest degree to the lower races of man, while it is rare or almost absent in the Caucasian (Aryan). "The predominance of the perforation (along with other degraded traits) in the chimpanzee and gorilla, as well as in the lower races of mankind, would suggest, if not a common ancestry in the remote past, at least some predisposing cause common to both the ape and the savage, and this connected with the use of the arm."

Of the long and elaborate paper on the "Stone Age in New Jersey," by Dr. C. C. Abbott, we can notice merely some of the author's conclusions as to the primæval tribes who made and used such instruments:—"What though the Mongol does resemble the American, does this in itself prove relationship?" And, also, it may be asked, *which* of the American aborigines, as they now are, does the Asiatic Mongol most resemble; or has each American tribe a representative in the other continent? Dr. Wilson asserts that "the theory of an aboriginal unity pervading our indigenous American race from the Arctic circle to Tierra del Fuego has been shown to be baseless;" but how can it be proved that the Indians apparently most nearly allied to Asiatic races are the oldest or *original* aborigines? We doubt that all American races are related, and if so different as Dr. Wilson assumes, who can demonstrate which type or pattern was the central, from which came the others that climate, food, and surroundings generally finally produced? "The Indian was once a palæolithic man, and, from whatever source he came, here advanced, without supernatural revelation or the missionary efforts of a superior people, to a condition which is best known as Neolithic." The memoir is profusely illustrated with figures of the stone weapons and tools described, and affords admirable evidence of the rapid progress lately made by American men of Science in investigating the condition of the pre-historic occupiers of the western continent.

Annual Record of Science and Industry, for 1876. Edited by SPENCER F. BAIRD. New York: Harper Bros. London: Trübner and Co.

THIS Annual Record appears to maintain its superiority over all publications of a similar nature. Alike in the selection and the arrangement of its materials, it has the advantage.

Among the recorded facts to which we may here briefly call attention is the hypsometric distribution of the Mollusca. It has long been known that on the slopes of mountains each species of plant has its peculiar zone, below or above which it does not prosper. A similar rule holds good with the terrestrial and fluviatile Mollusca. In the Central Pyrenees and the Alps M. P. Fischer has recognised five zones, each characterised by a species of *Helix*.

The subject of "seasonal dimorphism" receives notice. Certain insect forms, lately regarded as distinct species, are now found to be merely varieties, dependent upon temperature or upon the time of the year at which they appear. Chrysalids of *P. marcellus*, if exposed to severe cold in an ice-house, became *P. Telamonides*. Certain Australian moths, unlike ordinary Lepidoptera, do not suck the honey from flowers, but perforate fruits to feed upon the juices. For this purpose their proboscis is strong and sharply pointed.

According to Dr. Otto Hahn the much-disputed *Eozoon Canadense* is a "myth founded on a mistaken conclusion as to the micro-geological character of certain serpentines." If this statement is true it will be a heavy blow and great discouragement to Principal Dawson and other surviving members of the Cuvierian school. At the same time the *Bathybius* is becoming extremely questionable. It appears to be little more than a flocculent deposit of sulphate of lime.

An extract from the "Transactions of the Norfolk and Norwich Naturalists' Society" gives the "dimensions of some of the famous and larger oak-trees in England." With all deference to the Society we believe it would not be difficult to select oaks larger than many which they have described. We may instance the "Major" and the "Shambles" oaks, in Sherwood; the "Shire" oak, at the junction of the counties of York, Nottingham, and Derby; and the "King" and "Queen" oaks, in Dunham Massey Park, in Cheshire.

A paragraph, the origin of which has been accidentally omitted, states that "A comparison of the observations of naturalists with the weather-charts published in Europe and America makes it now seem certain that the weather immediately prevailing, and not that which is about to come in the near future, is the element which decides the movement of the greater number of migratory birds." This observation strips the migration of birds of no small part of the mystery in which it has been wrapped by unscientific writers.

The decrease of the birds of Massachusetts, both as regards numbers and species, has been examined by Mr. J. A. Allen. The mischief, for such it is, is ascribed in part to wanton shooting.

The well-known water-weed, *Elodea Canadensis*, is described by MM. Stein and Hanstein as having the power of evolving oxygen, or perhaps rather ozone, in considerable abundance. It may thus play an important part in burning up organic pollutions introduced into streams and water-courses, and in preventing—as Dr. Geisler has shown—the development of the lower forms of animal and vegetable life.

In an account of the controversy on Archebiosis, taken from the "British Medical Journal," we notice that the name of Dr. Bastian has been twice misprinted as Dr. Bartian.

Prof. Cope's theory of Evolution is noticed, but scarcely explained with the needful clearness. He considers that Darwin's doctrine of Natural Selection "has a secondary position in relation to the *origin* of variation which Lamarck saw but did not account for, and which Darwin has to assume in order to have materials from which a 'natural selection' can be made." The author takes a very just view of the influence of Cuvier, which retarded the progress of philosophical zoology for at least half a century. Prof. Cope, in his "Origin of Genera" and "Method of Creation," points out that the most nearly related animal forms present a relation of repression and advance, or a permanent embryonic and adult type, leaving no doubt that the one is descended from the other. This relation was termed *exact parallelism*. It was also shown that if the embryonic form were the parent, the advanced descendant was produced by an increased rate of growth, which phenomenon was called *acceleration;* but that if the embryonic were the offspring, then its failure to attain to the condition of its parent is due to the supervention of a slower rate of growth: to this phenomenon the term *retardation* is applied. *Inexact parallelism* is the result of unequal acceleration or retardation. Additions appear either as *exact repetitions* of pre-existent parts or as *modified repetitions*, the former resulting in simple, the latter in more complex, organisations.

We can, however, no further multiply extracts from this interesting volume, which everyone connected with scientific pursuits ought to read for himself.

Transactions and Proceedings of the Royal Society of New South Wales, for the Year 1875. Edited by A. LIVERSIDGE. Sydney: Richards.

To the scientific societies of the Colonies we look naturally and mainly for a very important class of investigations which they alone can furnish. We expect full accounts of the local fauna and flora, as well as of the palæontology, geology, and mineralogy of their respective regions. Nor are we altogether disappointed. The present volume contains a most interesting paper by S. H. Wintle, on the "Stanniferous Deposits of Tasmania." In this island tin appears to be abundant. In Mount Bischoff, Mount Ramsay, Wombat Hill, Mount Housetop, and in a variety of other localities, it occurs chiefly as ruby tin-ore. Bismuth is also found in Mount Ramsay, in a lode from 30 to 40 feet in width, which has been traced to a considerable distance. Wolfram, chrome-iron, titaniferous iron-sand, carbonate and sulphide of copper, appear also to abound. The climate and the face of the country offer considerable obstacles to the exploitation of these mineral treasures. At Mount Bischoff it is said to rain nine months in the year, and the forests are dense, dark, and compact to a degree almost unexampled. "The moisture is sufficient to render the country a fit habitation for a species of land lobster, whose circular mud-built walls and burrows are found everywhere."

Prof. Liversidge communicates a most valuable memoir on the "Minerals of New South Wales," which will be highly prized both by scientific mineralogists and by technologists.

The remaining pages in the volume are of less general interest. It is to be hoped that the Society will persevere in labouring upon the rich harvest-field of facts spread out before it. It has only to make use of its opportunities to win a high standing for "Transactions," and earn the gratitude of scientific men throughout the globe.

Journal of the Royal Society of New South Wales. 1876. Vol. x. Edited by A. LIVERSIDGE. Sydney: C. Potter.

THE Anniversary Address, delivered by the Rev. W. B. Clarke, Vice-President, contains some useful remarks on the necessity of Specialism in Science, and especially in what he rightly considers the chief function of the Society—the study of the Physical History of Australia.

In a paper on the "Origin and Migrations of the Polynesian Nation," by the Rev. Dr. Lang, the author maintains that the Polynesian nation is of Asiatic origin and of Malayan race, and was separated from the rest of mankind at a period of the earliest antiquity; that spreading gradually over the Pacific Islands they reached Easter Island, and from thence the western coast of South America, whence they gradually became scattered over all the western continent.

An essay on "Meteorological Periodicity," by the Government Astronomer, Mr. H. C. Russell, is exceedingly interesting. The author examines the various periods or cycles which have been put forward by meteorologists. He mentions that in Tasmania a biennial cycle, consisting of a wet and a dry year alternately, was traced and found to recur regularly for about twenty-five years. Then, however, two wet years occurred together (1848 and 1849), followed by two dry ones, since which Tasmania has had an uncertain rainfall.

A triennial period was suggested by a Mr. Tebbutt, from observations taken at Windsor, in New South Wales, and it has been also traced to the climate of Ceylon. The author does not consider the evidence in favour of an eleven years' cycle—corresponding with the supposed period of the solar spots—to be at all conclusive. There is, at least as far as Australia is concerned, much to be said in favour of a nineteen years' period. The author further draws attention to the influence of cosmical causes in modifying the climatic conditions of our earth. Thus the earth may, and probably does, pass through regions of unequal meteoric density, the heating power of the sun being thus temporarily diminished or increased. The fall of temperature in February and May, and its increase in August and November, producing in the latter month the phenomenon known as the "Indian summer" in America, and as "St. Martin's summer" in Europe, are instances in point.

The Rev. W. B. Clarke contributes a valuable paper on the "Effects of Forest Vegetation on Climate," and shows clearly, from a mass of evidence collected with great patience and judgment, that to denude a country is to destroy its fertility, to expose it to the destructive alternation of flood and drought, and to injure severely its sanitary condition.

We regret to find that the Zoological and Botanical Section of the Society held no meetings during the year. Surely there is an almost unlimited amount of work awaiting its attention.

Transactions and Proceedings of the Royal Society of Victoria. Vol. xii. Melbourne: Stilwell and Knight.

THIS volume contains no small amount of interesting matter. The important question "Is the *Eucalyptus* a fever-destroying tree?" is discussed at some length by Mr. J. Bosisto, and the conclusion drawn is affirmative. The immunity of the Australian continent from fever, as compared with other countries of similar climate, is remarkable, and that in spite of grievous sanitary neglect in many towns. "The various fever-types as found existing among us at times appear malignant, arising either from importation or from the existence of bad sanitary regulations; but medical testimony is that their virulence is meteor-like, and dies at its opening day. No credit can be taken for any improved sanitary condition of our surroundings by ourselves in our towns and cities; the influences operating there *entice* the fever-germ to fructify and abound."

Mr. F. J. Pirani contributes a memoir on "Some Processes of Scientific Reasoning." The following passage may interest some of our readers:—"Chemistry is almost entirely based on ideal construction. We popularly employ the term 'gold' to denote various objects which possess certain properties of weight, colour, &c.; but the gold of the chemist is an ideal conception, bearing the same relation to real gold as a geometrical sphere does to a real sphere."

"Notes on the Discovery of some Keys in the Shore Formation of Corio Bay, near Geelong," by Mr. T. Rawlinson, conveys the impression that in 1845 or 1846 a bunch of keys, of modern make, had been found *in situ* in a shelly bed on the shore, at the depth of about 15 feet below the surface. A note by Mr. R. C. Gunn, F.R.S., however, renders it probable that the keys, though found at the bottom of the shaft, must have been accidentally dropped in from above. The lime-burner who found them admits that he did not pick them out of the stratum of shells.

Mr. R. Etheridge, jun., contributes a paper on the Upper Palæozoic Polyzoa of Queensland.

Mr. A. M. Smith's "Notes concerning the Phenomena of the Approach and Recession of Bodies under the Influence of Radiant Energy" summarise the history of the Radiometer down to June, 1875.

The papers on "Surcharge of the Bullion Assay," by Mr. R. Barton, and on a "Proposed New Method of Weighing applicable to the Gold Bullion Assay," by Mr. G. Foord, though interesting, cannot be rendered intelligible without the accompanying diagrams.

Papers and Proceedings and Report of the Royal Society of Tasmania, for 1875. Hobart Town: Mercury Office.

The papers here presented to the learned world relate principally to botany, malacology, and geology. In a discussion which took place after reading the Rev. J. E. T. Wood's paper, on the "Fresh-water Shells of Tasmania," some interesting statements were made concerning the date of extinction of the moa (*Dinornis*) of New Zealand. The Governor, Sir Aloysius Weld, announced that he had been the first European who had visited the Kaikora country, in the southern island, and that he had been warned by the natives to beware of approaching the moa from behind, as it would kick, and might probably break his leg. They thus showed their acquaintance with the habits of birds of the ostrich family. With reference to the gigantic extinct eagle (*Harpagon Moorei*) he was told by an old chief that on the tops of the mountains an enormous bird, of a rufous colour, built its nest, and that in their forefathers' time it sometimes descended suddenly, and was large enough to carry off a good-sized boy or girl. Mention was made of *Scalaria Australis*, a sea-shell which yields a beautiful purple dye. It will be interesting to ascertain whether this colour is identical with that formerly obtained from certain molluscs inhabiting the Mediterranean, and which was known as Tyrian purple.

Dr. G. Bennett describes the "Frilled Lizard" (*Chlamydosaurus Kingii*) of Queensland. This creature has the curious habit of sometimes standing on its hind legs, and in that position walking, or rather hopping, like a bird. The lace lizard (*Hydrosaurus varius*) has a similar habit. The reader will be at once reminded of certain extinct saurian or sauroid beings which seem to have held an intermediate position between true reptiles and birds, and to have occasionally at least resorted to bipedal motion.

Dr. J. Milligan has presented to the Royal Society his valuable local herbarium, which we devoutly hope will be extended and carefully preserved. It is perfectly humiliating to hear of the fate of the Colonial Herbarium at Cape Town, which is being allowed to perish from damp and the ravages of insects.

We learn that a number of Australian trees introduced into the Isle of Arran are found capable of bearing the winter in the open air. The Australian palm (*Corypha Australis*), the silvered tree-fern of New Zealand, and the great Australian bush-fern were quite untouched by the frost.

A specimen of argentiferous galena, of Tasmanian origin, has been found to yield over 60 per cent of metal, rather more than half of which is silver.

The Royal Society of Tasmania evidently displays a very creditable amount of activity, though we regret to perceive, from the financial department of the Report, that "some members have not yet paid their subscriptions for the past year, and several are even in arrear for former years." It is sad that a Society which has such fine opportunities for research, and which numbers some most zealous members, should be hampered in its career by the want of funds.

Catalogus Polyglottus Historiæ Naturalis. By Prof. C. J. Wheeler. Chicago: S. J. Wheeler.

This singular work gives the names of certain animals, plants, and minerals in Latin, Spanish, French, English, and German, all arranged in parallel columns. Singularly enough many of the English names are incorrect, literal translations from the German being given. Thus no English writer would use "bear-pavian" as a synonym for *Cynocephalus porcarius*, or would call the common badger a "badger-dog." Porcupine and hedgehog are not synonymes for one and the same animal. The wren and the hedgesparrow, again, are two distinct birds. There is also a difference between the misteltoe, or missel thrush, and the fieldfare: the former is the *Turdus viscivorus*, and the latter the *T. pilaris* (Linn.). The common European viper is never known in England as "crossed adder" or "copper snake." *Lampyris noctiluca*, the glow-worm, is not called "John's beetle," nor did we ever hear it spoken of in Germany as "Johanniskäfer," a name we always knew to be applied to *Rhisotrogus solstitialis*. "Purple emperor" is the English name not of *Argynnis Paphia*, but of *Apatura Iris*. Many more mistakes of a similar kind might be enumerated if it were necessary. Accuracy is of course the one thing needful in a work of this class, and if found wanting it will only serve to promote misunderstanding and confusion.

Statistics, Medical and Anthropological, af the Provost-Marshal-General's Bureau, derived from Records of the Examination for Military Service in the Armies of the United States, during the late War of the Rebellion, of over a Million Recruits, Drafted Men, Substitutes, and Enrolled Men. By J. H. Baxter, M.D. In two volumes. Washington: Government Printing-Office.

The work before us consists of two goodly quartos, of about 600 pages each, profusely illustrated with diagrams. The Introduction gives an account of the working of the conscription system during the American civil war, and of the difficulties encountered by the medical officers in judging of the physical fitness of a recruit. The methods adopted for examining the soundness of the men are next described. Then follows a comparative view of the instructions issued by the United States Government and by the principal Governments of Europe for the guidance of the medical officer in the examination of recruits, every particular being given in detail. A great part of the work is devoted to a comparison among the men of different nationalities who enlisted in the United States Army, as regards height, weight, girth of chest, and liability to various diseases. The value of the results thus obtained entirely depends on the question whether the recruits examined can be considered as fair average representatives of their respective nationalities—a point on which, in some cases, very grave doubts may be entertained. The work, however, will be of very great value to the medical authorities of every army.

*The Influence of English Quakerdom upon German Culture, and upon the Anglo-Russian Project of a Universal Church.** By Bruno Bauer. Berlin: Grosser.

We have here a work published, it would seem, in the year 1878, and certainly deserving profound attention. To discuss a politico-ecclesiastic treatise in the peaceful pages of the "Quarterly Journal of Science" is of course out of the question; but we recommend Herr Bauer's views to the careful consideration of our political, religious, and social contemporaries. We were about to say that some of them might possibly have their eyes opened, but we remember that the infatuated are always able to interpret both facts and arguments in favour of their own prepossessions. To the author we would merely say that he has seen much, but not all, and that England has before now recovered from hallucinations not less threatening.

United States Commission of Fish and Fisheries. Part III. Report of the Commissioner for 1873-4 and 1874-5. Washington: Government Printing-Office.

This Report comprises an inquiry into the decrease of the food-fishes, and an account of experiments on the propagation of food-fishes in the waters of the United States. Among the interesting appendices we find a treatise on the condition of the fisheries among the ancient Greeks and Romans, and on their mode of salting and pickling fish. The share which the seas and rivers may be made to take in contributing to human subsistence was thoroughly understood in the days of classic antiquity. No fewer than four hundred kinds of fishes are mentioned by Greek authors—a proof of the great attention paid to the population of the waters. Artificial pisciculture, an art which is now only beginning to revive, was extensively practised.

Further sections give the statistics and other facts connected with the most important fisheries of the North Atlantic and of the Arctic Ocean, such as those of Norway, Sweden, Denmark, Britain, and Russia. The amount of valuable products thus obtained and the magnitude of the interests involved will be surprising to the general public. Thus in the year 1872 1,210,000,000 lbs. of herrings were taken in the Bay of Malanger, on the Norwegian coast. At Söndmöre the yield of the spring cod-fisheries was nineteen and a half millions of fish,—110,000,000 lbs. of liver, or at least 55,000,000 lbs. of oil, and 39,600,000 lbs. of roe. An especial notice is given to M. Sar's new theory of the migrations of the herring. He denies that it at any time of the year inhabits the deep-sea bottoms, where its favourite food, small oily crustaceans, cannot be found. During the summer it lives scattered, in the open seas, between Iceland, Scotland, and Norway, and approaches the Norwegian coast in a south-easterly direction at the beginning of the spawning-season. The Nordland *great* herring lives, in Sar's opinion, to the north-west of Nordland and Finmarken, but somewhat nearer the coast. The periodicity in the herring fisheries, the occurrence at certain intervals of years unusually productive, is a question still in dispute. It would be very rash to deny the possibility of such a phenomenon.

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* **Einfluss des englischen Quäkerthums auf die deutsche Cultur, und auf das englisch-russische Project einen Welt-kirc.**

The artificial cultivation of carp in ponds, as carried on in Holstein and East Prussia, is described at some length. We have heard it maintained that land laid out in well-arranged carp-ponds will yield a larger return than the same superficies devoted to grazing oxen or sheep. The profits in Silesia are given as about £7 10s. per acre.

All persons who take an interest in fish, whether zoological, economical, or gastronomical, will find this volume most instructive reading. Great credit is due to Prof. Spencer F. Baird for the zeal with which he pursues the important investigations with which he has been entrusted.

The Lazy Lays and Prose Imaginings. Written, printed, published, and reviewed by W. H. HARRISON. A.D. 1877 (Popular Chronology). A.M. 5877 (Torquemada). A.M. 50,800,077 (Huxley). London: 38, Great Russell Street.

WE have here a somewhat singular book, partly in prose and partly in rhyme, sometimes comic and sometimes sentimental. On its cover it bears emblazoned a griffin who is to keep watch on the Arimaspians of the nineteenth century—*i.e.*, American publishers. The author gives, among other things, instructions how to "double the utility of the printing-press." The simplest way to effect so desirable an end would be, in our opinion, to print less rubbish, in the prevalent shapes of novels, stump-orations, biographies, quack advertisements, and the like, so that books and papers worth reading might stand out more distinctly before the world. Mr. Harrison unfortunately takes a very different plan: he aims simply at economising paper by introduction of characters which would none of them take up more room than does the letter "i." We can see not the least good in his proposals. The introduction of a new character, which would certainly not be adopted all the world over, would retard instead of promoting civilisation, and would be particularly unfortunate at a time when there is some hope that the Germans may abandon their peculiar type, and thus place themselves in easier mental communication with the rest of cultivated nations. Our present characters, however irrational their origin, which is a totally irrelevant consideration, have the incalculable advantage that they are distinguishable from each other with little strain to the eye, and that very difference in size to which Mr. Harrison objects is therefore a valuable attribute. Letters formed on the author's plan would require much closer examination to distinguish one from another, and would, from their very trifling difference in size and shape, cause the eyes to "swim," occasioning serious injury to sight in people of studious habits.

"Why should any word," asks Mr. Harrison, "be more than one or two syllables in length?" And yet he is a poet! We have always protested against the length of the technical terms introduced into the sciences under pretext of "significance." But a language framed on our author's principles would rival in "mad monotony" the "Mission-room" bell which is at this very moment throbbing and tingling through our brain.

Mr. Harrison's strictures on some of the materialists of the day who yet cannot agree what matter is are much more valuable than his proposed printing-reform. His poems sometimes recall those of Hood.

Does Vaccination afford any Protection against Small-Pox? By T. B. Sprague, M.A., a Vice-President of the Institute of Actuaries.

Most persons are aware that compulsory vaccination is one of the "burning questions" of the day, and that, instead of being calmly discussed in medical and sanitary circles, it is taken up by political agitators as a point of their creed. On the one hand we have the orthodox medical practitioners, who as a body put a somewhat exaggerated faith in the prophylactic virtues of cow-pox. On the other hand is arrayed a league who consider vaccination as not merely futile and dangerous, but when enforced by law as an outrage on their civil and religious liberties. But there is still a third party, who, whilst utterly scouting the vested rights of disease and considering the State fully justified in enforcing precautions for the promotion of public health, do not feel altogether satisfied with the evidence adduced in favour of the efficacy of vaccination. They ask how is it that we have still small-pox epidemics, assuming almost pestilential proportions, if vaccination is indeed a safeguard? There are very few persons who do not undergo the operation once in life, and multitudes are re-vaccinated whenever the disease breaks out afresh in the neighbourhood which they inhabit. It is also declared that in the years 1815 to

1835, when vaccination was comparatively rare, small-pox epidemics were commonly spoken of as a thing of the past, and a pitted face among young people was almost unknown. These facts have not been satisfactorily explained, and any additional light is therefore welcome. Mr. Sprague, however, after a careful examination of the statistics of the case, draws no very decided conclusion. He thinks the inferences condemnatory of vaccination founded by Dr. Keller on the vital statistics of the *employés* of the Austrian State Railway scarcely warranted by the figures, but admits that the Doctor has established that "the rate of mortality among the vaccinated persons who were attacked was quite as heavy as that among the unvaccinated." In English statistics, and in the comments made upon them, he can "see nothing that at all explains why, notwithstanding the introduction of compulsory vaccination, the deaths from small-pox should have risen in the year 1872 so far beyond their number in any of the previous seventeen years."

Mr. Sprague is a spelling reformer, and a friend of Mr. H. Pitman; consequently the orthography of his pamphlet reminds us of our old acquaintance the "Fonetic Nuz."

Dynamics, or Theoretical Mechanics. In accordance with the Syllabus of the Science and Art Department. By J. T. BOTTOMLEY, Lecturer and Demonstrator in Natural Philosophy in the University of Glasgow. London and Glasgow: W. Collins, Sons, and Co.

THIS book belongs to "Collins's Elementary Science Series," and is specially prepared to meet the requirements of Science Classes under the "Science and Art Department." In the Introduction the author refers to a late change in nomenclature. "The name *Mechanics*," he remarks, "which properly denotes the science of machines, and was used by Newton in that sense, came for a time into use, instead of the appropriate word *Dynamics*, for the science which treats of force, and under that name—*i.e.*, Mechanics—there was a peculiar cross-division of the subject into Statics and Dynamics, in which the proper signification of the latter name was altogether departed from." We fear there is, in the present day, too strong a tendency to abolish names, inappropriate, indeed, if we look to their derivation, but which are thoroughly understood in favour of such as are more "significant" and etymologically more correct. It is a singular fact that the terms "mechanical" and "dynamical," now regarded as synonymous, were at one time treated as decidedly antithetical.

The work before us is divided into eight chapters, treating of the measurement of time, space, mass, velocity, acceleration, and momentum; of force, and the composition and resolution of forces; of the properties of matter; of gravitation; of the equilibrium of simple mechanical arrangements; of falling bodies; of curvilinear motion; of work and energy. The author, laudably enough, advises his readers to make themselves familiar with the metric system of weights and measures, but we fear that he is too sanguine in holding that these must shortly become universally employed.

In the "Exercises" at the end of the book we find the very remarkable statement that "a gallon of water weighs 18 lbs." This is doubtless a typographical error, but it calls for prompt correction. The work is well arranged, and the explanations lucid.

A Treatise on Chemistry. By H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S., Professors in the Owens College, Manchester. Vol. I. The Non-metallic Elements. London: Macmillan and Co. 1877.

It is now some years since a treatise on chemistry of any magnitude has been published in this country, and all students of chemistry will welcome the volume of Profs. Roscoe and Schorlemmer as a real addition to their science. Embracing the newest discoveries and the newest methods, it aims to give a clear and detail account of Modern Chemistry, taken in its broadest significance. When we call to mind the large experience and untiring industry of the authors, both as regards original research and professorial teaching in lecture-room and laboratory, we have a right to expect a very able treatment of the subject at their hands. This expectation is fully realised.

Out of the 750 pages of which this first volume consists, 40 are given to an historical introduction, 54 to the general principles of the science, 610 to the non-metallic elements, and 46 to crystallography. The Introduction gives a brief but very lucid account of the rise and progress of the science, traced from the earliest times to discoveries of Priestley, Scheele, and Lavoisier, and the development of the atomic theory by Dr. Alton. The more prominent facts in the history of organic chemistry are also detailed. In the second section the laws of chemical combination are clearly explained, and the manner in which the combining weights of bodies are obtained from the analysis of compounds. The kinetic theory of gases and diffusion and effusion are discussed, and a brief but all-sufficient account of chemical nomenclature concludes this section.

The non-metals are divided into the four usual groups, according to their atomicities, and they are described in the order in which they there occur. The account of ozone contains all that we know on the subject, and is fuller than is usually found in our manuals. It is called "Active Oxygen," and is represented as $O_3 = 47{\cdot}88$, having a density $= 23{\cdot}94$. That is, three volumes of oxygen form two volumes of ozone, and, when acted upon by potassium iodide, one-third of the ozone is used up in liberating the iodine, and the remaining two-thirds go to form ordinary oxygen—

$$O_3+2KI+H_2O=O_2+I+2KOH.$$

Group 5.—Besanez has recently shown that ozone is invariably formed when water evaporates, and it is mainly to this source that its presence in the air has to be traced. Ozone has been recently used for bleaching engravings which have been discoloured by age. They are rolled up and placed in a glass globe containing water and moist phosphorus. Ozone has also been used for the purpose of oxidising alcohol, C_2H_6O, to aldehyde, C_2H_4O, a substance employed in the manufacture of aniline-green. In the next section the proofs of the composition of water are elaborately discussed, and an excellent engraving shows the completest form of apparatus for the oxide of copper synthesis. A best method of exploding a mixture of oxygen and hydrogen is shown in Fig. 68; and of the energy developed we are told that 1 grm. of hydrogen, in burning to form water, produces an amount of energy sufficient to raise 24,577 kilogrms. through the space of 1 metre. An ingenious apparatus for showing that sulphur dioxide contains half its volume of sulphur vapour is figured on page 307. An exhaustive account of the manufacture of sulphuric acid, with diagrams of the newest forms of apparatus for its distillation, forms a prominent feature of the article on Sulphur. This is followed by a description of the "Chlorides and Bromides of Sulphuric Acid"—names, we think, which have not been very happily chosen. A detail account of carbon, and its compounds with hydrogen, chlorine, oxygen, sulphur, and nitrogen, conclude the volume.

Throughout the book the style is very lucid. The engravings, which are quite equal to those we meet with in French and German scientific works, are directly taken from photographs of apparatus in actual use, and the entire get up of the book is excellent. We trust that the second volume will soon make its appearance.

An Elementary Treatise on Physics, Experimental and Applied. Translated from Ganot's "Eléments de Physique," by E. Atkinson, Ph.D. Eighth Edition, Revised and Enlarged. Longmans. 1877.

This well-known work, which has long since established its reputation, has reached an eighth edition. Since its first appearance it has, in each succeeding edition, been enlarged and rendered more complete. The present additions include sixty-two new illustrations, and about sixty pages of new matter. It presents in all respects remarkable completeness; there are numerous tables, and four coloured plates, including two which represent respectively the isogonic and isoclinic lines for the year 1860. Few changes have been made in the earlier parts of the book. In "Sound," however, we find several additions notably relating to the synthesis of sounds, Kundt's determination of the velocity of sounds, and Kœnig's manometric flames. In Heat, the subject of Radiant Heat is treated more fully, an account of Crookes's Radiometer and the theory of its action, Bunsen's Ice-Calorimeter, and the sources of cold. An interesting description of Horn's method of determining the mechanical equivalent of heat will be found on p. 410.

In Light, the construction and use of Hedley's Reflecting Sextant is given, and the measurement of small angles by reflection from a mirror. The Spectroscope is treated much more fully than before, also the whole subject of the eye. The chapters on Magnetism are enlarged and much improved since the earlier editions, and the plates showing isogonic and isoclinic lines are beautifully clear and intelligible. In Electricity, the applications of the discharge to the firing of mines has been introduced; also a woodcut, and brief account of Leclanché's battery (unaccompanied, however, by the equation which expresses the change within the battery). A graphic representation of the heating effects in a circuit is given on p. 710, and the mechanical effects of the battery on p. 718. The science of Electro-dynamics is treated at greater length than before, and several new woodcuts have been introduced to illustrate this portion of the subject. The sounder telegraph receives no more than a passing notice; a woodcut in the next edition would be acceptable in this connection. Becquerel's electric thermometer is described in detail and figured, and the subject of animal electricity receives greater attention than hitherto. In the Meteorology and Climatology a description, accompanied by a good engraving of Secchi's Meteorograph, will be found; also Tyndall's researches on the influence of aqueous vapour on the radiation of heat, and on the blue colour of the sky. The volume is concluded by 221 well-selected problems and examples in various branches of Physics, the answers to which are given.

We congratulate Prof. Atkinson on the completeness and thoroughness of his work in connection with the new edition of Prof. Ganot's "Physics."

Stability of Motion. By E. J. Routh. (Adams's Prize.)

This is an essay of somewhat uneven interest. The able and elegant discussion of the conditions that an equation may have no positive roots and no imaginary roots with the real part positive is very interesting, both in method and result; but the remainder of the work takes (perhaps necessarily) too much the form of enquiry into special and limited cases. We are inclined to think that the line of investigation as slightly sketched in Chapter VIII. would have yielded results of more general theoretical interest. But the subordination of theoretical developments to practical application seems to us the defect of this laborious and original essay.

Spherical Harmonics. By the Rev. M. M. Ferrey.

An exceedingly clear and well-arranged account of the leading properties of these functions, with continual reference to their application to the theory of the potential. Possibly too great care has been devoted to giving algebraically simple proofs of the preliminary propositions, which might have been exhibited in closer connection by a free use of Green's theorem. One is surprised, too, to find no mention of Prof. Maxwell's representation of solid harmonics as derived from $\frac{1}{r}$ by axis-differentiation. But if these are faults they are alone. The concluding chapter on Ellipsoidal Harmonics, as the author happily terms Lamé's functions, is an especially good introduction to this elegant extension of the spherical analysis.

Inductive Metrology; or the Recovery of Ancient Measures from the Monuments. By W. M. Flinders Petrie. London: Hargrove Saunders. 1877.

This work gives a very elaborate comparison of the measures ot all the great nations of antiquity with those of modern times. Measurements from more than 600 buildings have been considered, and as many as 10,000 measurements have been examined. Among the results obtained may be mentioned the proof of "the exact identity of the American mound-builders' unit with the Hebraio-Persian cubit, which had a wide and ancient diffusion in the old world. The close similarity of the Mexican unit with the widespread 21·4 unit of the old world; and the similarity also of the pre-historic British, and Christian Irish unit to this. The close similarity of the Phœnician unit to a principal unit of pre-historic British remains, and also to the Polynesian unit. The identity of the Pelasgic with the Etrurio-Roman foot. The continuance of the Romano-British units into mediæval times, the resemblances being generally inexact, and far within the probable errors. Also the similar continuance of the classical units into the Mohammedan times in Turkey and Persia."

Incidents in the Biography of Dust. By H. P. MALET. London: Trübner. 1877.

THIS work reminds us in its style of some of Victor Hugo's rhapsodies concerning matter and force. A few quotations from the work will be more effective in showing its general character than any more formal notice, for which we have neither the inclination to write nor the space to give:—

"Air is composed chiefly of oxygen, hydrogen, and carbonic acid gases."

"Air and water are active, dust is the passive element."

"Pressure condenses dust; dust condenses heat."

"Heat acts on the dust."

Southern Stellar Objects for Small Telescopes, between the Equator and 55° South Declination. By J. E. GORE, Asst. Engineer, India. Lodianet. 1877.

THIS carefully compiled work embodies the results of observations made in the Punjaub with an achromatic of 3 inches aperture and four feet focal length. All the best catalogues of southern stars have been consulted. The work will be of much use to observers in India.

Physiography and Physical Geography. By the Rev. ALEX. MACKAY, LL.D., F.R.G.S. London: William Blackwood. 1877.

IN September, 1876, the Committee of Council on Education decided that Physical Geography, as defined in the Science Directory, was not a subject in which the special aid of the Science and Art Department should be continued. But the Committee substituted for it *Physiography*, a subject designed to embrace "those external relations and conditions of the earth which form the common basis of the sciences of nautical astronomy, geology, and biology, as treated in the Science Directory." Dr. Mackay's work was written in order "to meet the wants of such an examination. It is a good book, but too overcrowded with facts. It is a condensation of what might fill a dozen volumes. In the short space of 143 pages it treats of Mathematical Geography; the Relation of the Solar System to the Universe; the Earth Viewed Individually; Configuration of the Surface; the Atmosphere; Climate; Electricity and Magnetism; Biology; Geology; Optical Instruments; How to Find the Distance of the Heavenly Bodies; Map Projections and Geodetical Surveys. Such extraordinarily condensed matter can never be properly digested, and it tends to foster that system of "cram" which is the curse of our modern education.

The London Science-Class Books. Elementary Series. Edited by Prof. G. C. FOSTER and PHILIP MAGNUS. *Thermodynamics*, by RICHARD WORMELL, D.Sc., M.A. *Astronomy*, by R. S. BALL, LL.D., F.R.S., Royal Astronomer of Ireland. London: Longmans and Co. 1877.

THE works of this series have special reference to science teaching in schools, but surely not to science as now taught in the generality of schools. If the time comes when there are regular science "sides" in schools, as we now have "modern side" and "classical side," these works will be invaluable, but they are far above the capacity of boys who are only able to devote two hours a week to science. Mr. Wormell's "Thermodynamics" is a very dry book, only suitable for the most advanced boys in large schools, who would use it in lieu of some mathematical work. It is absurd to put into the hands of boys, who do not understand the construction and use of a thermometer, a work which embodies in a condensed form some of the mathematical deductions of Clausius, Thomson, and Clerk Maxwell.

Professor Ball's "Astronomy" is written in a clearer and more interesting style, and will be decidedly useful for those who, having some knowledge of mathematics, desire to make themselves acquainted with the first principles of astronomy.

The Voyage of the "Challenger": The Atlantic. A Preliminary Account of the General Results of the Exploring Voyage of H.M.S. "Challenger" during the Year 1873 and the early part of the Year 1876. By Sir C. WYVILLE THOMSON, LL.D., F.R.SS. L. and E., F.L.S., &c. In Two Volumes. Published by Authority of the Lords Commissioners of the Admiralty. London: Macmillan and Co.

IN forming an opinion of these two volumes regard must be had to the limitations which appear upon the title-page. The account of the exploring expedition here given is only "preliminary."

As we are reminded in the preface, some years must elapse before the facts ascertained can be all thoroughly digested. "The observations are still unreduced, the chemical analyses are only commenced, and there has not been time even to unpack the natural history specimens." Hence to give anything like a detailed account of the additional data which have been acquired by the "Challenger" expedition, or of their bearings upon the various problems of physical geography, must be quite impossible. To pronounce, therefore, upon the value of the expedition from a mere perusal of the present volumes, and to declare it a failure, either relatively or positively—as some persons are doing—appears to us in the highest degree rash and presumptuous.

Another point to be remembered is, the work before us embraces only the less interesting portion of the expedition, the exploration of the Atlantic. The transactions of the second and third years, in the Indian and Pacific Oceans, cannot as yet be placed before the public in their entirety.

To form some idea of the amount of work actually performed we must bear in mind that the explorers took up 362 special observing stations, as nearly as possible at equal distances, and at each made the following observations :—

"The exact depth was determined. A sample of the bottom, averaging from 1 oz. to 1 lb. in weight, was recovered by means of the sounding instrument, which was provided with a tube and disengaging weights. A sample of the bottom water was procured for physical and chemical examination. The bottom temperature was determined by a registering thermometer. At most stations a fair sample of the bottom fauna was procured by means of the dredge or trawl. At most stations the fauna of the surface and of intermediate depths was examined by the use of the tow-net variously adjusted. At most stations a series of temperature observations were made at different depths from the surface to the bottom, and samples of sea-water were obtained from different depths. Atmospheric and other meteorological conditions were carefully observed and noted ; the direction and rate of the surface current was determined, and at a few stations an attempt was made to ascertain the direction and rate of movement of water at different depths."

But it must not be thought that the duties of the expedition were limited to the observations just detailed. The islands and the coasts of continents visited were also to be explored from several points of view. It was especially enjoined upon the voyagers to "obtain information about the vegetation of oceanic islands." These, as is very justly remarked in the code of official instructions drawn up by a committee of the Royal Society, "are in many cases the last positions held by floras of great antiquity, and, as in the case of St. Helena, they are liable speedily to become exterminated, and therefore to pass into irremediable oblivion when the islands become occupied." We may even ask whether it would not be possible to secure an agreement among civilised nations against turning loose pigs, goats, or rabbits on any island not inhabited by carnivorous animals. Had it not been for goats much of the original flora of St. Helena would be in existence at the present day. The terrestrial faunæ of the islands and coasts visited were doubtless examined in the same manner as the floræ.

Thus, in addition to details of what may be called sea-work, we find a very complete account of the Bermudas, the Falkland group, Tristan d'Acunha, Ascension, the Açores, besides notices of Madeira, the Cape Verde islands, the Rocks of St. Paul, and Fernando Noronha. In this latter island, a Brazilian convict station, distant about 180 miles from the mainland, and hence likely to present interesting correspondences or differences, the expedition met with a strange reception. The governor would allow them to land, offered them horses and guides and every possible accommodation, but on condition that no scientific work was to be done. "Captain Nares asked, if we saw a butterfly might we not catch it, but he said he would prefer that we should not!" This was a great disappointment, as some of the party had intended to prepare a monograph of the natural history of the island. On approaching Bahia they encountered one of those strange hosts of butterflies which are occasionally met with on the coasts of Brazil, soaring out seawards. They belonged to the slight and delicate genus *Heliconia*, and fluttered all day over the ship and over the sea as far as the eye could reach, like the flakes in a heavy snow-storm. This curious phenomenon is not by any means perfectly understood.

At Sto. Amaro the party met with an ornithological incident which may be worth mentioning. "As the truck ran quickly down the incline the swarthy young barbarians, attracted by the novelty, crowded round it, and suddenly the agonised cries of a child, followed by low moanings, rang out from under the wheels, and a jerk of the drag pulled the car up and nearly jerked us out of our seats. We jumped out and looked nervously under the wheels to see what had happened, but there was no child there. The young barbarians looked at us vaguely and curiously, but not as if anything tragical had occurred, and we were just getting into the car again, feeling a little bewildered, when a great green parrot in a cage close beside us went through another of his best performances in the shape of a loud mocking laugh. A wave of relief passed over the party, and the drivers expressed to the parrot their sense of his conduct, I fear strongly. For a fortnight dredging was carried on with great success in the shallow waters of the bay, almost every haul bringing up large numbers of fine tropical shore forms, when one of the men on leave was suddenly and fatally attacked with yellow fever, and it was deemed prudent to weigh anchor at once.

Dredging and trawling in the deep sea is an operation not exempt from difficulty and even danger, and often disappoints the eager naturalist. Between St. Thomas and the Bermudas "the large iron dredge which we were using in preference to the trawl caught upon a rock, or a mass of coral, and brought a sudden strain upon the dredge-rope; and before the rope could be veered or any other steps taken to relieve the strain, the hook of the foremost span carried away, and the leading block which was hooked to it flew back and struck William Stokes, one of the sailor lads, with such violence that he was driven against the ship's side. His thigh was broken in two places, and he was so seriously injured otherwise that he never recovered consciousness, and died a few hours afterwards." An accident, unattended with any tragical features, occurred on the run between Bahia and the Cape. "The trawl was lowered, and on heaving in it came up apparently with a heavy weight, the accumulators being stretched to the utmost. It was a long and weary wind-in on account of the continued strain; at length it came close to the surface and we could see the distended net through the water, when just as it was leaving the water and so greatly increasing its weight the swivel between the dredge-rope and the chain gave way, and the trawl with its unknown burden quietly sank out of sight. It was a cruel disappointment,—every one was on the bridge, and curiosity was wound up to the highest pitch: some vowed that they saw resting on the beam of the vanishing trawl the white hand of the mermaiden for whom we had watched so long in vain; but I think it is more likely that the trawl had got bagged with the large sea-slugs which occur in some of these deep dredgings in large quantity, and have more than once burst the trawl-net."

We find no mention of portions of the skeletons of the higher animals having been brought up by the dredge, save some sharks' teeth and the ear bones of whales fished up in one case from the depth of 2275 fathoms. No saurian or ophidian remains are mentioned as having been met with.

The floating islands of sea-weed, the Sargasso sea of the old Spanish navigators, offer in their fauna some curious examples of protective resemblance. "Animals drifting about on the surface of the sea with such scanty cover as the single broken layer of the sea-weed must be exposed to exceptional dangers from the sharp-eyed sea-birds hovering above them and from the hungry fishes searching for prey beneath; but one and all of these creatures imitate in such an extraordinary way both in form and colouring their floating habitat, that we can well imagine their deceiving both the birds and the fishes."

A curious observation, recorded in a former work of the author's and confirmed during the cruise of the "Challenger," is the absence of eyes in certain deep-sea animals, whilst in others they are fully developed. In *Ethusa granulata*, a stalk-eyed crustacean, well-developed eyes are found in specimens taken in shallow water. In deeper water, from 110 to 370 fathoms, the eye-stalks are still present, but the animal is apparently blind, the stalks ending not in eyes, but in mere round calcareous knobs, whilst at still greater depths, from 500 to 700 fathoms, the eye-stalks themselves lose their specific character and become fixed. Here, then, as the author remarks, we "have a gradual modification, depending apparently upon a gradual diminution and final disappearance of solar light." But in *Munida*, a form from equal depths, the eyes are "unusually developed, and apparently of great delicacy."

It is certainly conceivable that as the light gradually decreases in amount one of the changes may take place ; the power of vision may fade away *pari passu* as no longer needed, and its organs themselves may ultimately become abortive, or, on the other hand, it may become more and more acute as the solar light declines, and, as Sir Wyville Thomson suggests, may "become susceptible of the stimulus of the fainter light of phosphorescence." But on what does it depend in any given case which of these two kinds of progressive modification shall take place ? Why do we find the influence of identical circumstances so different, if not diametrically opposite ? This is a question which the biologist has often to put to himself in vain.

A most interesting phenomenon met with in the Falkland Islands may require a somewhat lengthened notice on account of its connection with the "glacial question." We refer to the so-called "stone-rivers," a natural feature not occurring elsewhere on such a scale. Many of the valleys in the East Island are occupied by pale grey masses, varying in width from a few hundred yards to a mile or upwards, and which, from a distance, simulate glaciers descending from the ridges and stretching down from the sea. They consist, however, not of ice, but of blocks of quartzite, from two, twenty feet in length by about half as much in width, resting irregularly upon each other, and supported by the edges and angles of those below. They are not weathered to any extent, though the edges and points are in most cases slightly rounded, and the surface also perceptibly worn, but only by the action of the atmosphere, is smooth and polished, and a very thin, extremely hard white lichen, which spreads over nearly the whole of them gives the effect of their being covered with a thin layer of ice." Water is heard murmuring down below, and occasionally becomes visible where the interval between the blocks is exceptionally large. At the mouth of the valley the section of the "stone-river" is like that of a large stone drain. The only difficulty in accounting for this phenomenon is, according to the author, the slightness of the slope ; that from the ridge to the valley not exceeding six degrees, whilst the inclination of the valley itself is only two or three, so that blocks of such a form are obviously unable to roll or slide down. How, then, is the effect produced? The beds of quartzite vary greatly in hardness; some crumbling readily away, whilst others scarcely yield at all to ordinary weathering. The softer portions being thus worn away, the harder beds are left as long projecting ridges along the crests and flanks of the hill ranges, and ultimately, being deprived of their supports, give way, and the fragments fall over upon the sloping hill-sides when they become imbedded in vegetation, a slight inequality in the surface of the turf merely indicating the blocks buried beneath it. From a variety of causes, the whole soil-cap, blocks included, gradually creeps down even very gentle slopes. Thus the alternate expansion and contraction of the vegetable mass, according as it is soaked with water or comparatively dry, gradually pushes the blocks downwards. The rain-water as it runs down clears away the movable matter before them, and the vegetable mould on which they rest "is undergoing a perpetual process of interstitial decay and removal." In this manner the blocks are gradually swept down the slope and collected in the valley.

The author gives analogous cases, though on a less striking scale, which he has observed in the West Highlands of Scotland and elsewhere. He adds:—"It seems to me almost self-evident that wherever there is a slope, be it ever so gentle, the soil-cap must be in motion, be it ever so slow; and that it is dragging over the surface of the rock beneath the blocks and boulders which may be imbedded in it; and frequently piling these in moraine-like masses where the progress of the earth-glacier is particularly arrested, as at the contracted mouth of a valley where the water percolating through among them in time removes the intervening soil. As the avalanche is the catastrophe of ice-movement, so the land-slip is the catastrophe of the movement of the soil-cap. As I have already said, I should be the last to undervalue the action of ice, or to doubt the abundant evidences of glacial action; but of this I feel convinced, that too little attention has been hitherto given to this parallel series of phenomena, which in many cases it will be found very difficult to discriminate; and that these phenomena must be carefully distinguished and discriminated before we can fully accept the grooving of rocks and the accumulation of moraines as complete evidence of a former existence of glacial conditions."

The theory thus suggested evidently demands very careful examination. Every geologist must have observed blocks embedded in vegetable mould at considerable distances from the rocks whence they were obviously derived upon slopes too gentle to admit of rolling or sliding and in situations where the accumulation of a head of water sufficient to bear them down seems utterly improbable. If they have travelled since these slopes can have been glaciated, if they are still travelling, Sir W. Thomson's theory must be accepted. But the movement is exceedingly slow, so as to render direct evidence of its existence difficult to procure. By putting forward this view, valuable as it is to all true geologists, the author will certainly offend that class of dogmatists who are now presuming to call themselves sceptics in geology.

But we must now turn to the author's general results as laid down provisionally. Till lately the most eminent naturalists, judging from such limited evidence as was at their disposal, were of opinion that life at the bottom of the sea was confined to a narrow border around the land, and that at the depth of about 100 fathoms plants disappeared almost totally and animal life became scarcer and scarcer, representing only such groups as are among the simplest in their organisation, whilst at greater depths—300 fathoms and upwards—organic life was absent, the physical conditions precluding the possible existence of living beings. Gradually, however, facts came to light which could not be reconciled with this view of the distribution of organic life. Samples of the sea-bottom brought up from the depths of the Atlantic by Brookes's sounding machine were found, on microscopic examination, to consist largely of the shells, entire or broken, of certain Foraminifera, which appeared to have lived where found rather than to have floated on the surface of the sea. Star-fishes with their stomachs full of deep-sea Foraminifera were brought up from depths of 1200 fathoms. Still this important evidence was scarcely received as sufficient. Men of science hesitated to admit the possibility of animals flourishing, not merely in the total absence of day-light, but under a pressure amounting at 1000 fathoms to 1 ton on every square inch of surface. The results obtained by the author and Dr. Carpenter during their voyages in the "Lightning" (1868) and the "Porcupine" (1869 and 1870) followed up on a far wider scale by the great expedition of the "Challenger," may be said to have fully decided this important question.

The views formerly entertained concerning the limitations of oceanic vegetable life are indeed unchanged, the author finding that vegetation is "practically limited to depths under 100 fathoms." Very few of the higher Algæ inhabit the surface of the sea, the chief exception being the gulf-weed, *Sargassum bacciferum*. Thus marine plant life may still be regarded as substantially littoral. But with animals the case is totally different. It is present at the bottom of the sea at all depths, though much less abundant in the deepest parts. Still, as well-developed members of all the marine invertebrate classes are found at all depths, their distribution does not appear primarily connected with any of the conditions immediately depending upon depth. It appears, however, that the oceanic fauna is chiefly confined to two strata; the one on and near the surface, and the other on or near the bottom. In the intermediate stratum the larger animal forms, whether vertebrate or invertebrate, are almost, if not entirely, wanting. The chief invertebrate marine groups, though all represented in the fauna of the abyss, occur in peculiar proportions. Mollusca (shell-fish), brachyurous Crustaceans, and Annelida are scarce, whilst Echinodermata and Porifera were largely preponderate. Below 500 fathoms the fauna presents the same general features in all parts of the world. Deep-sea genera are either cosmopolitan in their distribution, or if differing in distant localities are to a marked degree mutually representative. It was anticipated that the deep-sea fauna would be more closely related than the littoral fauna to the faunæ of the secondary and tertiary periods. This expectation has been realised to a certain extent, though the number of types discovered which had been supposed extinct is not large. The most characteristic deep-sea forms, including those most closely related to extinct types were found in the greatest abundance and in the largest size in the Southern ocean, and the "general character of the faunæ of the Atlantic and of the Pacific gives the impression that the migration of species has taken place in a northerly direction." This capital observation, we must remark, confirms the converse conclusion formed as to the distribution of land-animals, which are supposed to have spread from the northern hemisphere southwards. In other words, terrestrial forms have originated in and have been distributed from the greatest mass of land, just as marine forms have originated in and have spread out from the greatest mass of water. The deep-sea fauna generally, as might be expected, resembles the littoral fauna of high latitudes, the physical conditions to which it is exposed being nearly identical. A new order of animals approaching to but still distinct from the Radiolarians has been discovered and has received the name "Challengerida."

No less important are the physical and hydrographical results. We find indeed, in the present volumes, no observations throwing any light on former westward extensions of the eastern continent or eastward prolongations of America. The question whether Atlantis was a large island south-westward of the Canaries, or a large and level tract reaching to the east of the Lesser Antilles, according to Mr. Belt's suggestion, remains precisely where it was. It is indeed remarked that the Açores form the culminating points of a plateau of considerable extent, and that the Bermudas present the appearance of an "atol." But it may now be considered as definitely established that the bed of the Atlantic is divided by an axial ridge, and its branches into three basins—the eastern, extending from the West of Ireland southwards nearly to the Cape of Good Hope, with an average median depth of 2500 fathoms; the north-western, occupying the great eastern bight of the American continent, and having the average depth of 3000 fathoms; and a third opening southwards and running along the coast of South America to Cape Orange, and having also an average depth of 3000 fathoms. The Atlantic, and even the North Pacific, may be, in fact, regarded as mere gulfs or prolongations of the great preponderating water-mass of the southern hemisphere, from which a cold under-current is steadily flowing northwards. This arrangement depends on the circumstance that the rainfall in the Southern sea is in excess of evaporation, whilst in the North Atlantic and Pacific the reverse rule holds good. The temperature of the Atlantic does not appear sensibly affected by any influx of cold water from the Arctic Sea. The greatest depth found in the Atlantic—3875 fathoms, an abyss in which Chimborazo would be "taken overhead"—was observed a little to the north of the Virgin Islands.

At all depths between 400 and 2000 fathoms the bottom of the Atlantic—save where the *débris* brought down by rivers or due to the disintegration of rocks interferes—is covered with "Globigerina-ooze," a calcareous deposit mainly produced by the shells of Foraminifera, but mixed with fragments of pumice, felspar, sanidine, angite, hornblende, quartz, leucite, and magnetite, generally in very small particles. As the globigerina-ooze is decomposed, it gradually passes into so-called "red clay," more or less homogeneous, which covers the floor of the deep depressions below the 2000-fathom level. In this substance recognisable mineral fragments were found in greater abundance. The pieces of pumice were most numerous in the neighbourhood of well-known volcanic centres, such as the Açores and Philippines, and they are supposed to be all due to sub-aërial volcanic action. It does not appear easy to identify any of the beds now in course of formation at the bottom of the ocean with the rocks belonging to any of the past geological periods. The rocks of the Mesozoic and Kainozoic series seem to have been formed in shallow water. The author believes, however, that the globigerina-ooze, if elevated and slightly metamorphosed, would yield a limestone very closely resembling a bed of grey chalk, whilst the "red clay," under similar circumstances, might approximate closely to one of the palæozoic schists. He does not wish, however, to offer a definite opinion until the comparative chemical and microscopic examination of the specimens dredged up is completed.

The volumes before us must produce a favourable impression upon every candid reader. Though necessarily "dry" in parts, they are perfectly free from any gratuitous parade of technicalities, as well as from "fine writing," and, above all, from any obtrusion of the author's personality. Sir Wyville Thomson sees, and tells us what he sees, without "posing" for our admiration. The work is also not like too many records of voyage and travel, ballasted with irrelevancies. If we further bear in mind that we have here merely an instalment, and that probably the less interesting part of the work done by the "Challenger," we must pronounce the results of the highest service to Science, and creditable to the Government which sent out the Expedition, to the illustrious Society by which it was planned, and to the eminent men by whom so arduous a task was successfully executed.

The Different Forms of Flowers or Plants of the Same Species. By CHARLES DARWIN, F.R.S. London: Murray.

THOUGH modestly proclaiming himself no botanist, Mr. Darwin continues to produce botanical researches whose sterling value must be admitted even by the most determined adherents of the old school of Natural History. The work before us treats of some very interesting points connected with the reproduction of plants. A certain number of vegetable species were grouped together by Linnæus as hermaphrodites, and amongst these are a class which Mr. Darwin studied and described some years ago as "dimorphic" and "trimorphic," and which have since been named "hetero-styled" by Hildebrand. In plants of this class there are individual flowers of two, or in other cases of three, forms, differing principally in the relative length of the pistils and stamens. A familiar instance may be found in the common cowslip, polyanthus, and auricula. In some of these flowers the globular stigma appears at the mouth of the corolla, whilst in others the stigma does not protrude, and in its stead appear the anthers as an annular tuft. These different forms of the flower are named by florists respectively "pin-eyed" and "thrum-eyed." The former type is named by our author the long-styled, and the latter the short-styled. These two kinds of flowers are never found upon one and the same plant. Mr. Darwin has investigated the meaning of this diversity, and finds that it is by no means accidental or unimportant. If the long-styled form is fecundated by the pollen of the short-styled form, or *vice versa*, we have complete fertility; the seed is abundant and good, and the author accordingly speaks of this as a "legitimate union." If, on the other hand, a long-styled flower is fertilised by the pollen of a long-styled flower, or if a short-styled flower is fecundated by the pollen of a short-styled flower, we have two cases of "illegitimate union," in which the seed produced is not merely much less in quantity, but inferior in quality.

There are other plants, again, in which we find not two, but three different forms, as in certain species of *Lythrum*, *Nesæa*, *Oxalis*, and *Pontederia*. Here, again, we mark the same difference between the effects of legitimate and of illegitimate union. The author remarks that there is a wonderfully close parallellism between the effects of illegitimate and of hybrid fertilisation. "It is hardly an exaggeration to assert that seedlings from an illegitimately fertilised hetero-styled plant are hybrids formed within the limits of one and the same species. This conclusion is important, for we thus learn that the difficulty in sexually uniting two organic forms, and the sterility of their offspring, afford no sure criterion of so-called specific distinctness. If anyone were to cross two varieties of the same forms of *Lythrum* or *Primula* for the sake of ascertaining whether they were specifically distinct, and he found that they could be united only with some difficulty, that their offspring were extremely sterile, and that the parents and their offspring resembled in a whole series of relations crossed species and their hybrid offspring, he might maintain that his varieties had been proved to be good and true species, but he would be completely deceived. In the second place, as the forms of the same trimorphic or dimorphic hetero-styled species are obviously identical in general structure with the exception of the reproductive organs, and as they are identical in general constitution (for they live under precisely the same conditions), the sterility of their illegitimate unions, and that of their illegitimate offspring, must depend exclusively on the nature of the sexual elements, and on their incompatibility for uniting in a particular manner. And as we have just seen that distinct species when crossed resemble in a whole series of relations the forms of one and the same species when illegitimately united, we are led to conclude that the sterility of the former must likewise depend exclusively on the incompatible nature of their sexual elements, and not on any general difference in constitution or structure. We are, indeed, led to this same conclusion by the impossibility of detecting any differences sufficient to account for certain species crossing with the greatest ease, whilst other closely allied species cannot be crossed, or can be crossed only with extreme difficulty. We are led to this conclusion still more forcibly by considering the great difference which often exists in the facility of crossing reciprocally the same species, for it is manifest in this case that the result must depend on the nature of the sexual elements, the male element of the one species acting freely on the female element of the other, but not so in a reversed direction. And now we see that this same conclusion is independently and strongly fortified by the consideration of the illegitimate unions of trimorphic and dimorphic hetero-styled plants.

It can scarcely be denied that these latest researches of Mr. Darwin have dealt a most serious, if not an absolutely fatal, blow at the so-called physiological test of species, and consequently at the line of absolute demarcation by which some naturalists still consider species—as distinct from varieties—to be bounded.

The Methods of Physical Science. A Lecture delivered at University College, Bristol, as Introductory to the Course of 1877-78. By SYLVANUS THOMPSON, B.Sc., &c. Bristol: T. Kerslake and Co. London: Longmans and Co.

WE have here a lecture which brings forward not one novel fact, and probably no conclusion which has not been arrived at and pointed out before, but which is yet of exceeding value as clearly and authoritatively summing up the methods by which alone scientific truth may be attained. These methods are too apt to be sneered at by mere paper philosophers, the professors of the so-called "Wissenschaft in der phrase;" they are practically overlooked by the young, the inexperienced, and the superficial, who have yet to learn the great lesson of thoroughness. Hence it is most appropriate that they should be fully explained and enforced at such a time as the opening of a course of study in a new institution for higher—and we hope for scientific—education.

Mr. Thompson naturally addresses himself to the student of physics or of chemistry, but he says little, if anything, which has not an equal claim of the biologist or the geologist, in short, of all who in any department are engaged in questioning nature.

The lecturer takes for his text, if we may use the expression, the "Rules of Philosophising" laid down by Newton in his "Principia."

I. "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances."

II. "To the same natural effects, therefore, we must, as far as possible, assign the same causes."

A striking example of the breach of these two rules is given by certain biologists in their attempts to deny intelligence to the lower animals. If a man acts in a certain manner to attain a given end, they declare he has been guided by intelligence, but if a fox or an ape proceeds in a manner totally analogous they then ascribe his conduct to *quasi*-intelligence, and thus assign different causes to the same natural effects.

The author refers to a "certain note of dissatisfaction at the methods of modern science, and that in quarters where it cannot be passed by; a dissatisfaction amounting almost to disgust. This "revolt from the experimental method" is manifested in the writings of Hegel, and finds an echo in a passage quoted by the author from Buckle's "History of Civilisation" (vol. iii., p. 379). We may even doubt whether Comte can be altogether acquitted of having given utterance to similar views. This same tendency to undervalue experiment and observation and to return to the vicious methods pursued in the dark ages may be traced in an advice given in a certain quarter to naturalists that they ought by all means to study metaphysics. But it is not from open attacks upon the inductive method that we have the most to fear. It is manifestly not merely the right but the duty of science to subject every phenomenon to the test of experimental study. No matter how difficult the question, or how much it may have been complicated by imposture, folly, and superstition, this obligation still remains. Yet we have men of science, so-called, who rest satisfied with superficial perfunctory researches, mere apologies for a foregone conclusion, and who, on the faith of their own "educated common sense," adjudicate not merely theories or hypotheses, but facts. What is still worse, these men make it their business to ridicule and disparage the earnest enquirer who is searching for the pearls which they trample under foot.*

Mr. Thompson gives a survey of "some of the more special methods of physical science which have been of service in the acquisition of facts." He enumerates—*Casual Observation, Methods of Comparison, Methods of Precision, Methods of Analogy, Methods of Hypothesis, and Residual Methods*, each of these being explained by reference to cases of its successful application. With the last-mentioned method he connects the names of Boyle, Wollaston, Faraday, and Brewster in the past, and that of Mr. Crookes in the present.

"As a mental and moral training," the author observes," "the pursuit of the scientific method is priceless." "He who will be warped by prejudice, by passion, or by fear cannot investigate nature rightly."

The students of the Bristol University College may well be congratulated on a teacher whose conceptions of the methods and the duties of science are so clear, so wide, and so lofty.

The Canadian Journal of Science, Literature, and History. Vol. XV., No. VI., July, 1877. Toronto: Copp, Clark, and Co.

THIS issue opens with an interesting paper on "Left-handedness," by Dr. D. Wilson, in which the important question is raised whether any phenomenon corresponding to the preferential use of the right hand in man can be traced in the inferior animals and especially in apes. It is curious that the handle of a bronze sickle found at the lake-dwellings of Möringen, on the Lake of Brienne, in Switzerland, is adapted for use with the right hand. But the deer-horn picks recovered from the "Grime's Graves" flint pits are not invariably right-handed. "Left-handedness," the author considers, "is inherited and transmitted, though in an irregular manner and with varying intensity; that the range of the influence, to whatever source we may trace it, affects other organs of the same side only partially and uncertainly." He declares that with the majority of mankind the left hand is systematically reduced to the condition of a comparatively useless member. He does not, however, give sufficient prominence to the fact that this uselessness among right-handed persons varies very considerably in degree. We have met with persons who, without any previous training, could on the very first attempt write legibly with the left hand, whilst others failed completely even after many trials.

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* This treatment of truth is not, it appears, confined to the *pachy*-dermata.

There is a valuable synopsis of the Flora of the Valley of the St. Lawrence and of the Great Lakes, by Dr. J. Macoun, the Botanist to the Canadian Geological Survey, giving the localities of each species and descriptions of the rarer plants.

Proteus, or Unity in Nature. By C. B. RADCLIFFE, M.D. Second Edition. London: Macmillan and Co.

WE do not wish to be irreverent, but this work irresistibly reminds us of an old wooden frigate hastily converted into an iron-clad in some real or fancied emergency. "Substantially," as the author tells us in his mythological and mystic introduction, "a second and much enlarged edition of a very immature work published under the same title more than twenty-five years ago," it is now refitted, and sent out to cruise against the doctrine of Evolution. He considers that much of what he has to say is likely to find "little favour in a materialistic age like the present." Has he yet to learn that between Evolutionism and Materialism there is no necessary connection? He gives us "traces of unity" in plants, in the limbs of vertebrate animals, in the appendicular organs of invertebrate animals, in the skull and vertebral column, in the animal as a whole, in plants and animals, in organic and inorganic forms. Passing from form to force, he lays before us traces of unity also in the various modes of physical force, in vital and physical motion, in the "vivifying power of light and heat," in the phenomena of "instinct," of memory, of imagination, volition, and intelligence, as well as in the personal, social, and religious life of man. All this is worked out with abundance of learning, with references to and quotations from numerous authors of all ages, though also with an obvious leaning to far-fetched analogies which almost reminds us of G. H. v. Schubert, or of Heinrich Steffens. But accepting in substance what Dr. Radcliffe has to urge, and without at all seeking to impair its value by facts of an opposite tendency, we have to inform him that this very unity is confirmation strong of the doctrine of Evolution! It is precisely what we should expect if the whole organic world had been developed from some common primitive form. It is precisely what we should not expect if each plant and each animal were of independent origin. Let us take a parallel case: the chemical unity of the heavenly bodies, as shown almost demonstrably by spectroscopic research and by the analysis of meteorites, is justly considered as a valuable piece of evidence in favour of the "nebular hypothesis," *i.e.*, the evolution of suns and planets out of one common material. It is strange to find an author, after elaborately collecting instances of unity in creation, breakind down the barriers between the plant and the animal,—a task already accomplished, —and even attacking that "which has been erected between the domains of organic and inorganic nature;" it is strange, we say, to find such an author suddenly arrested by the dim and evanescent boundary line which alone lies between those respective groups which we call species, and which he considers as primordially distinct. He declares, indeed, that "the doctrine of unity in diversity, and diversity in unity, has little or no bearing, direct or indirect, upon the doctrine of Evolution." If so, why does he append his attack upon Evolution to a work devoted to pointing out "traces of unity?" His only argument against Organic Evolution, beyond his declared ignorance of "any evidence which ought to lead to a different conclusion" seems to lie in the following passage:—"And what other conclusion can be drawn from the infertility of mules than this—that there *is* a barrier between different *species*, even between those which are most closely akin to each other, by which they are kept apart most effectually." That such lines can still be written by a man of learning is to us a mystery. Shall we never be delivered from the errors deduced from that unfortunate fact that the best-known hybrid, the mule, happens to be barren? The fertility of the léporide—the hybrid between the hare and the rabbit—has been recently most fully confirmed in a paper in "Les Mondes," an organ bitterly hostile to Evolutionism. In a recent and valuable monograph on the American bison we have satisfactory evidence that this animal can produce permanently fertile hybrids with the domestic cow; yet the two belong not merely to distinct species, but to distinct genera! But to any candid enquirer we think the recent researches of Mr. Darwin on the reproduction of plants must supply full proof that the difficulty of producing hybrids, and their infertility where it exists, is due to causes very different from a supposed barrier between species, and supplies no valid argument against Evolution. Dr. Radcliffe concludes that "each creature was created as a necessary part of a great whole, perfect in itself, and perfect in its relations to other creatures and to the universe to which it belongs!" But he gives us no proof of this alleged perfection, no standard by which it can be judged or recognised. If each animal was necessary, why do we see so many blanks in the animal series which have not been filled up? Why do strange plants and animals, artificially introduced into any country, often displace its original flora and fauna? In fact, there is scarcely a phenomenon connected with animal geography, or with the powers and functions of animals, which can be fairly reconciled with the creed of the old Natural History as here summarised by our author.

We have no sympathy with materialists, but we cannot forget that every important step in the development of science has been met with charges of materialism and atheism. From all we can gather from this work, its author, both by disposition and by the character of his studies, is better fitted to deal with words or with dreams than with things, and would shine much more in fanciful interpretations of myths than in the sober domains of Natural Science.

The Geological Record for 1875. An Account of Works on Geology, Mineralogy, and Palæontology, published during the Year. Edited by W. Whitaker, F.G.S. (of the Geological Survey of England). London: Taylor and Francis.

In few sciences is an annual record or general summary of work done so needful as in geology. So wide and so manifold is the subject, so closely blended with physics and chemistry upon one side and with biology upon the other, and so numerous are the organs in which matter of geological interest may first see the light, that without some journal like the one before us it is scarcely possible for the student to keep himself abreast of the current of discovery. We hope, therefore, that there may be no question as to the success of so necessary an undertaking.

The information given is arranged under the following heads:—Stratigraphical and Descriptive Geology, Physical Geology, Applied and Economic Geology, Petrology, Mineralogy, Palæontology, Maps and Sections, Miscellaneous and General. Most of these classes are again subdivided, and each such division is placed in the hands of a responsible sub-editor, To extract even a tithe of the interesting matter contained in the work would be of course impossible, but we may, as specimens, lay a few fragments before our readers. G. Krefft, in his "Remarks on Prof. Owen's Arrangement of the Fossil Kangaroos," considers that the whole of the Australian marsupials, extinct and living, are descendants of an extinct animal comprising the dental structure both of the carnivorous and herbivorous sections of the sub-order, and of which *Thylacoleo* was the last representative.

O. C. Marsh, in a paper on a "New Order of Eocene Mammals" contained in the "American Journal" (vol. ix., p. 221), gives an account of the Tillotherium, which combines the characters of Carnivora, Ungulata, and Rodentia. This is another of those discoveries which so powerfully support the doctrine of Evolution, presenting, as they do, characters in combination which have been since separated out and distributed among different animal groups.

Rütimeyer's work, "Traces of Man in Swiss Interglacial Deposits" ("Verhand. Nat. Ges. Basel," vi., 333 to 342), describes wooden rods, artificially sharpened, and remains of coarse wickerwork, found in the interglacial lignite along with known extinct animal species (elephant, rhinoceros, cave-bear, *Bos primigenius*, &c.

Brief notice is taken of Principal Dawson's "Address on the Origin and History of Life on our Planet." The author concludes that "the introduction of new species of animals and plants has been a continuous process, not necessarily in the sense of the derivation of one species from another, but in the higher (?) sense of the continued operation of the cause or causes which introduced life at the first." This seems very like the demand for a continuous miracle. Against this view the objection urged against Darwin weighs with tenfold force. For if no one has witnessed an ape becoming a man, still less has anyone witnessed a new animal condensing out of the atmosphere, crystallising out of the waters, or emanating from the ground. Principal Dawson holds also that "Palæontology furnishes no direct evidence, perhaps never can furnish any, as to the actual transformation of one species into another, or as to the actual circumstances of the creation of a new species; but the drift of its evidence is to show that species come in *per saltum*, rather than by any slow and gradual process."

S. H. Scudder's important paper on "Fossil Lepidoptera" is somewhat severely abridged. We are told that the author "discusses the comparative age of fossil butterflies, the probable food-plants of Tertiary caterpillars, the present distribution of butterflies most nearly allied to the fossil forms, and the affinities of certain fossil insects which have been referred to butterflies," but there is given no summary of the conclusions arrived at.

The evidence of glaciation in India is becoming stronger, but the details bearing on this question, as well as interesting accounts of the coal-fields and the other mineral wealth of the Empire, are to be met with in the Reports of the Geological Survey of India.

The geology of Africa is still in a very imperfect state. The British provinces in the South ought at any rate to be thoroughly explored.

Mr. J. C. Brown has written an important memoir on the hydrology of South Africa, discussing the causes of its present aridity, and suggesting appropriate remedial measures.

The geology of Australia is engaging the attention of a greater number of explorers. Mr. J. Bonwick estimates the carboniferous area of New South Wales at 15,419 square miles, and expects that part of the comparatively unknown western interior of the colony may prove to be carboniferous. The coalfield of New South Wales and Queensland he considers to be the western edge of a larger area now under the Pacific.

A paper contributed by Mr. J. Goodhall to the "Transactions of the New Zealand Institute" is of great importance as regards the early existence of mankind—or at least of some tool-using animal—on our earth. During excavations in the city of Auckland a tree-stump was discovered in its natural position, upright, with roots penetrating the surrounding clay, and covered by about 25 feet of volcanic *débris*, consisting of stratified beds of ooze and volcanic ash adjacent to a volcanic centre. The clay in which the roots occur rests upon Tertiary rocks, and is 10 to 15 feet thick. The stump is said to be a "tea-tree" (*Manuka*), and to have been cut with a tool. The author holds that it proves the existence of man long before the period indicated by the traditions of the Maories of their advent in this island, and at a period before what is probably the oldest volcano in Auckland became extinct.

At Mercedes, near Buenos Ayres, human bones have been discovered at a depth of 4 metres in undisturbed Quaternary beds, mixed with burnt wood, earth, and flint-implements; also bones of about fifteen species of mammals, mostly extinct.

This interesting volume finishes with "Addenda," a "Supplement," and a "Postscript," which reminds us of the headings "Finally," "Lastly," and "To conclude," in a Puritan sermon.

Is Scientific Materialism Compatible with Dogmatic Theology? The Inaugural Address delivered before the Literary and Philosophical Society of Liverpool on the Opening of the Sixty-Seventh Session, October 1, 1877. By JOHN DRYSDALE, M.D., President of the Society, and author of "The Protoplastic Theory of Life." Liverpool: Adam Holden.

THE Literary and Philosophical Society of Liverpool, as we were on a former occasion compelled to remark, seems to have a strong propensity for the discussion of subjects which can scarcely be held to fall under its legitimate cognisance. Dr. Drysdale's able and interesting address may doubtless rank as "philosophical" in the more recent and etymologically more correct acceptation; but when the literary and philosophical societies of England were founded, and when their aims and functions were defined, the word "philosophical" was, we submit, used in a sense which now survives mainly in the phrase "philosophical instruments." In the first quarter of the present century, and even down to, say, the year 1840, the word "philosopher" was the exact equivalent of the German "Naturforscher," and of the recent American word "scientist." Hence were it not that the Society bears also the very vague name of "Literary," Dr. Drysdale's address could only be regarded as a breach of order. As it is the discussion of his views, in a strictly scientific organ, is scarcely within the limits of possibility.

The author answers his own question—the momentous character of which we do not seek to under estimate—in the affirmative. An evolutionist, a believer in the mere phenomenal character of life and mind, and a disbeliever—if we understand him aright—in teleology, at least as it has been hitherto taught; he accepts, "on supernatural authority, the knowledge of the existence of personal conscious-thinking beings other than man, and whose substance is non-material, and that man in a personal, conscious, and responsible state shall live again for ever." He shows that man's utmost power and skill applied to the interpretation of the Universe only leave him the choice between materialistic Pantheism, as expounded, *e.g.*, by Haeckel, Deism, and "negative Atheism.*" After pointing out the utterly unsatisfactory character of the two former hypotheses he declares his conviction that the third "is not incompatible with dogmatic theology, and not even unfavourable for the reception of it. Our attitude in this category may be compared to that of the humble publican who prostrates himself on the floor of the Temple, and cries aloud in agony, overwhelmed by the cruel and crushing power of natural laws, and the blank emptiness of all visible signs of the presence of God in nature. Is the cry to go up to Heaven for ever and no answer to be vouchsafed? No! a thousand times No! For from the depths of the unseen world the voice of the Almighty Himself has been heard, declaring His will and His nature and purpose, so far as seemed to Him good, and as we are fitted to comprehend. . . It is only in the external bearing of revelation, in contrast to science, that we are at present concerned, and therefore we must speak of it explicitly, as the special revelation bound up with the history of Moses and of Christ. Whatever may be the defects of historic evidence in comparison with the proofs, repeatable at will, of physical science, it is obviously all we have to depend on, and by it the whole of dogmatic religion as something apart from all other knowledge and truth must stand or fall. For the—to us—supernatural revelation is inseparably bound up with the historic truth of the persons and miracles of Moses and of Christ; the so-called internal evidence of the truth of the teachings of Christ, valuable as they may be, are wholly insufficient of themselves; and no rational thinker can accept the Gospels without accepting the miracles, which form an essential part the narrative, or rather, they are the essence of christianity itself. This is conclusively shown by the destructive criticism of the German school represented by Strauss and Baur. That the moral elements of Christianity could survive the destruction of the historic personality of Christ and the whole supernatural element is a mere chimera; the notion never had any rational foundation, and if any belief in it ever existed it must have been dispelled by Strauss's last book. It is only, therefore, as a supernatural element of actual knowledge that Christian dogmatics can take a place in our knowledge, and prevail over, or rather, supplement the conclusions of scientific materialism. Without the supernatural element there is, as far as I can see, no choice except between Pantheism and negative Atheism; for the flimsy barrier of Deism is swept away at once whenever the hold on the supernatural is abandoned."

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* By this term we presume the author means not the dogmatic denial of a god, but merely a confession of inability to infer from the Universe the existence of a First Cause, personal, just, and benevolent.

Now, we are certainly not prepared to say that Dr. Drysdale's views in this respect are not here and there open to criticism, but they involve, at least, no fundamental absurdity, and it will be worth carefully considering whether they do not open out the only possible way of escape from the difficulties which we encounter whenever—as none of us can help at times attempting—we would rise from the details of research to first principles. It must be confessed that the Universe, as we now behold it with the eye of unaided reason, is a ghastly spectacle. Time was when we fancied we saw in the woods and the fields realms of peace and joy. We knew, of course, that the hawk and the weasel were preying upon the small birds, that the spider was sucking the life-juices of the fly, and that the ichneumon larva was feasting among the vitals of the caterpillar. But all this seemed merely an exceptional disturbance of the general bliss. Now all this is changed; whether Natural Selection be a true cause of the formation of species or not; nay, whether species have been evolved at all, or primordially created, we cannot get rid of the Struggle for Existence, which, like Lytton-Bulwer's "Dweller on the Threshold," haunts every man whose eyes have been opened. Well might Winwood Reade, quailing before this phantom, and finding no solution to the mysteries of the Universe, exclaim "Creation is murder."

We need scarcely say that Dr. Drysdale, in accepting the Christian revelation as fully compatible with the results of the latest advances in science, makes the necessary reservation, old as the days of Galileo, that the Scriptures are not a code of geology, physics, biology, or psychology. This simple principle that where matters of observation and experience are touched upon Revelation speaks a merely popular language—with the same right as the most precise of us all still speaks of the sun's rising and setting—does away with the necessity of all attempts to reconcile "geology and Genesis," and at the same time cuts away the ground from beneath those who contend for the rejection of revelation on the ground of its technical inaccuracies.

We have no doubt Dr. Drysdale will encounter some rough usage. Those who presume to say that the object of Evolutionism is to undermine religion will doubtless brand him as an infidel. On the other hand, he will be denounced as an abject slave of superstition by such men as Posner, who, in a passage here quoted, declares that we "must deny God, and trample the Cross under foot, before we can become even scholars, far less masters in Natural Science." With this vicious and unphilosophical outbreak Dr. Drysdale deals with just severity. He admits that thoughts of First Causes and teleological ideas "tend to check the progress of investigation at numerous points." The fact that the majority of scientific men are not religious in the conventional and restricted sense of the term cannot, he thinks, be denied. But this adds very little strength to the fanatically intolerant position of Posner, since "the field of science is now so enormous that a man must not only give himself up wholly to it, but even to a very small part of it, in order to make any new conquest for the domain of knowledge. Hence even an incapacity to judge of religious truths." That there is no essential antagonism between religion and science the author considers is proved "by a long roll of illustrious names, from Newton to Faraday." To all the numeroust emptations to premature hypothesis and baseless speculation the infidel is at least as open as the believer. "For it is possible for the latter to free his mind from all bias, and push back creative interference as far as the search for phenomenal causes can possibly demand, and then he is in a better position for calm judgment than the fanatical infidel-evolutionist, for example, who is compelled to find a natural origin of life, and thus is tempted to fantastic speculation on the spontaneous generation of completely organised living beings with a life-history in a few hours from fermenting chemical compounds. Even on the descent of man the believer can rise to a calm and more unprejudiced standpoint than the Häckelian." Our author also thinks that the moral causes of mental disturbance must be taken into account. He remarks that "envy, jealousy, hatred, and prejudice are as rife among men of science as among other men, and these dim the pure love of truth, which is the essential condition of all discovery in science. Where are the qualities to counteract these most likely to be found? Surely among those desiring honesty, honour, truth, love, esteem, veneration, and beauty, to which the highest ideal is presented in Christian dogmatics!" Inverting Posner's position our author asks: "May we not, therefore, say, No one can truly become a master in science unless he first takes up the Cross and blends indissolubly the perfect love of truth, as a moral duty, with the love of truth in nature, which is the foundation of all scientific method?"

United States Geological Survey. Miscellaneous Publications. No. 8. *Fur-bearing Animals: A Monograph of North American Mustelidæ.* By Elliot Coues. Washington: Government Printing Office.

We have here a most valuable monograph of a group of animals not merely important on account of the furs they yield, but interesting to the naturalist from many strange points in their character and habits. Our notice is first claimed by that most unsavoury beast the skunk, *Mephitis mephitica*. The genus *Mephitis*, and the American species in particular, their outward and inward structure, geographical distribution, and habits are described with elaborate care. The offensive secretion whose horrible properties have not been exaggerated by popular tradition, and the excretory organs or "atomisers" for its ejection and distribution in the atmosphere, have been examined by Dr. J. M. Warren, who, undeterred alike by the dreadful odour, and by the possibility of anti-vivisectionist denunciations, examined the glands in a living specimen which had been placed under the influence of chloric ether. The fluid does not appear to have been hitherto submitted to chemical examination, but we trust that before long some enterprising student will ascertain its properties and its "constitution." It is described as being phosphorescent, of an altogether indescribable odour, at once pungent, penetrating, and intensely nauseating. Both men and dogs have been permanently blinded in consequence of a drop having come in contact with the eye-ball. According to Sir John Richardson a dead skunk, thrown over the stockades of a trading post, produced instant nausea in several women in a house with closed doors upwards of a hundred yards distant. The vulgar notion that the offensive liquid is the urine of the animal is utterly unfounded. As regards the distance to which this fluid can be propelled the author remarks that "the squirt reaches several (authors say from four to fourteen) feet, while the *aura* is readily perceptible at distances to be best expressed in fractions of a mile.

The authority and the reasoning of Waterton, notwithstanding, there is no doubt but that this creature is fully aware of the offensiveness of its secretion to other species and uses it as a defensive weapon. "Its heedless familiarity, its temerity in pushing into places which other animals instinctively shun as dangerous, and its indisposition to seek safety by hasty retreat, are evident results of its confidence in the extraordinary means of defence with which it is provided." In the woods of Nicaragua Mr. Belt repeatedly observed the calm assurance of the skunk, who "goes leisurely along holding up his white tail as a danger-flag for none to come within the range of his nauseous artillery."

But this animal, it is now known, possesses a far more dangerous attribute than that of the emission of an execrable odour, but which certainly contributes nothing to his defence. His bite is usually fatal, inducing a disease very similar to if not identical with canine rabies. One remarkable point is that the skunk possesses this power, not when himself attacked with a fatal disease, as is the case with the dog, but when apparently in a normal state of health. Some unsolved questions here present themselves. *Rabies* is said to be not uncommon among wolves on the European Continent, and very general among jackals in India. Do these animals invariably, or even commonly, die of the disease, or are they, like the skunk, capable of inflicting a poisoned bite when in good health? Again, is there any authenticated case of idiopathic hydrophobia in a cat or in any other feline animal? The skunk displays what may be called gratuitous malignity. Its bite is inflicted not in defence of itself or its young, but without any provocation and without any prospect of advantage. A trapper or a traveller may be asleep on the ground of his camp-fire when a prowling skunk will stealthily approach the sleeper and bite the lobe of his ear or his little finger. Under such circumstances as this the majority of the cases of skunk-bite occur, and there is only one recorded instance which has not terminated fatally. *Mephitis gorilla*, the Californian species, is equally to be dreaded. There almost seems to exist among the Mustelidæ a "superfluity of naughtiness" such as no other of the lower animals display, but which are shown by the ermine, the stoat, and the pekan in their wholesale slaughter of birds, &c., far more than they require for food, and in the useless mischief committed by the wolverine or glutton. Of the extraordinary intelligence and thievish propensities of this strange animal we find here many most surprising but apparently authentic instances. We are of opinion that no one can read the work of Dr. Coues without finding a new light dawn upon him both as o the intellectual and the moral faculties of "brutes."

Bulletin of the United States Entomological Commission. On the Natural History of the Rocky Mountain Locust and on the Habits of the young or unfledged Insects as they occur in the more fertile country in which they will hatch the present year. No. 2. Washington: Government Printing Office.

This report, drawn up by the Commissioners, Messrs. C. V. Riley, A. S. Packard, Jr., and Cyrus Thomas, gives a full account of the habits of the *Caloptenus spretus*, the Rocky Mountain Locust, so as to facilitate arrangements for their destruction. The deposition of the eggs, their curious arrangement in the soil, and the method in which the young locust ruptures the shell are clearly described and figured. An appended map shows the country east of the Rocky Mountains which has been already overrun, and from which they will too probably spread still further to the eastward. The best remedy, in our opinion, would be the introduction of live hedges and hedgerow trees capable of affording abundant cover to insectivorous birds in place of the rail and wire fences so common in the Western States, and which find their thoughtless advocates in England.

A Guide to the Determination of Rocks; being an Introduction to Lithology. By EDWARD JANNETTAZ. Translated from the French by G. W. PLYMPTON, C.E., A.M. New York: D. van Nostrand.

THIS work, in its original French form, came under our notice some time ago ("Quarterly Journal of Science," v., p. 109), on which occasion we ventured to pronounce it a not unsuccessful attempt at supplying an important deficiency in geological literature. Until the student is able to identify with moderate accuracy the rocks which he meets with he has not yet learnt the alphabet of the science, and is unable either to make original observations or to verify the researches of others. Any work which may enable him to recognise the different rocks he sees when out on a geological ramble contributes something towards transforming him from a mere reader into a worker in the science. It is very true that the determination of geological species is not always easy,—instance the discrimination of granite and gneiss. Still, if the student fixes in his mind the chemical and physical characters of the rocks, or rather of their chief mineral constituents as here laid down, and continually applies this knowledge in practice, he will find the difficulty not insurmountable.

The first part of the book gives the properties of the principal minerals which compose rocks. The second is devoted to a description of the rocks themselves; whilst the third lays down systematic procedures for the practical determination of any rock which falls into the hands of the student. Part fourth, the appendix, translated from M. Stanislas Meunier's "Cours Elementaire de Geologie Appliquée," consists of a dichotomic system for the determination of rocks. Having no respect for dichotomy as a key to classification, we must yet admit its extreme value for the diagnosis of species.

It may not be out of place to notice the exceedingly elegant binding of this little volume.

CORRESPONDENCE.

THE HALO IN THE ZENITH, AND THE HALO OF 90°.

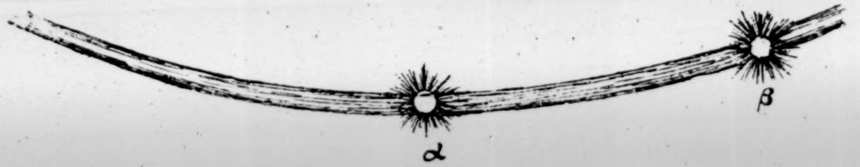
By the Rev. S. Barber, F.M.S.

Sir,—Both of these rare phenomena having occurred during the present month (September, 1877) a brief notice is now given by an observer in the neighbourhood of Eythorne, near Sandwich, Kent.

The halo in the zenith was seen on September 8th, from about 4.30 to 5.30 p.m. The weather was fine, and the sky marked by a number of bands and patches of cirrhus cloud. A single parhelion appeared, in an opening of the clouds, where the sky was clear. This was distinctly chromatic, and globular in form. It lay to the north of the sun.

The peculiarity in this appearance was in the bright colouring of the halo (which was, indeed, almost as distinct as that of a rainbow) and in the character of the originating medium. It is well known that the cirrhus cloud—consisting, as it does, of snow-crystals floating at a considerable elevation—often gives rise to halos and prismatic bands of light. On this occasion the line of the halo was intersected by well-defined bands of cirrhus, lying almost parallel to one another. There was no break in the arc of the halo, which was as visible upon the blue sky as on the cloud. From this observation we may make the inference that the actual limits of the cloud constituents are not always confined to the visible limits of the cloud itself. This perhaps may be thought to be only another way of asserting the fact that, even in an apparently clear sky, there may be above us vast quantities of snow crystals and vapour particles.

In regard to the weather succeeding this appearance, we may note that within forty-eight hours there were heavy showers, and one very violent squall.



Halo of 90° with Parhelion. α. The Sun. β. Parhelion.

The halo of 90°, which was seen from the same neighbourhood as the preceding, occurred at about 12.30 on the morning of Tuesday, September 25th. This was also attended by a single mock-sun, through which the arc of the halo passed (see figure). This line also passed through the sun itself,—or, rather, seemed to branch out from the sun, on either side, and made a circuit of the heavens. It was not prismatic, but of a grayish tint, and was broken in its outline, in the part of the sky opposite the sun, by patches of cirro cumulus.

It is remarkable that the weather following, in this case, was finer than had been experienced for some time previous to the manifestation.

Until October 8th, when there was a gale, the air was clear and fresh, and sunny during the day, with the usual autumn mists in the morning and evening (wind easterly for the most part). It may be remarked, however, as I have noted on previous occasions in this Journal and elsewhere ("Quart. Journ. of Science," Nos. 26 and 41, and "Nature," March

23, 1871), that optical phenomena of the kind here referred to indicate a transition state of the weather; and this inference has recently been again substantiated (October, 1877) by the occurrence, on two successive days, of imperfect parhelia at the end of a period of fine weather, and before the advent of storms and rain. That this is the true prognostic value of these appearances will scarcely be disputed now by meteorologists. How to account for the fact that ice-crystals become chiefly visible at these times of transition does not appear easy. It is well known that in the finest weather abundance of crystals may be found at high elevations, even with a clear sky; but it seems perfectly clear that optical phenomena do not always attend them.

I venture, therefore, to propose the following explanation of the fact that halos generally attend a "transition" state of the atmospheric vapour, viz., that *they result from the transmission of the solar rays through their substance while in a partially melted state*, or, rather, while slightly melted on the surface.

It must be observed that in dry frosty weather the longer ice remains exposed to atmospheric influences the more uneven its surface becomes, by the accretion of vapour upon its prominences, and consequently the less capable it becomes of giving a clear refraction and exhibiting chromatic effects. Now if we allow that, previous to the appearance of a halo or parhelion, the floating ice may have been present in this condition, a rise of temperature would produce the optical phenomena referred to, by making the refraction more perfect; and, on the other hand, a fall of temperature sufficient to freeze would have the same effect, by origination of new ice-prisms from existent watery vapour.—I am, &c.,

S. Barber.

Tilmanstone, Sandwich.

SCIENTIFIC NOTES.

THE most important scientific achievement we have to record in the present number of the "Quarterly Journal of Science" is that of the liquefaction of oxygen, which was accomplished on the 22nd of December by M. Raoul Pictet. When a gas is compressed to 500 or 600 atmospheres and kept at a temperature of $-100°$ or $-140°$, and it is allowed to expand to the atmospheric pressure, one of two things takes place—Either the gas, obeying the force of cohesion, liquefies and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder. For some time past M. Pictet's object has been to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception. The apparatus he employed in his experiments may be briefly described as follows: Two pumps, such as are used in M. Pictet's ice-making apparatus, are coupled in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first commences with a tube 1·1 metre long and 12·5 centimetres in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of this liquid rapidly sinks to $-65°$, and even to $-73°$, the extreme limit allowed. Through this tube of sulphurous acid passes a second smaller tube, of 6 centimetres diameter and the same length as the envelope. These two tubes are terminated by a common base. In the central tube M. Pictet compressed carbonic acid produced by the reactions of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer of 1 cubic metre capacity. At a pressure of from 4 to 6 atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube, 4 metres in length and 4 centimetres in diameter. Two pumps, coupled together like the first, exhaust carbonic acid either from the gasometer or from the long tube full of liquid carbonic acid. The ingress to these tubes is governed by a three-way tap. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube: it is situated between the condenser of carbonic acid and the long tube. When this screw valve is closed, and the two pumps draw the vapour from the liquid carbonic acid contained in the tube 4 metres long, the greatest possible lowering of temperature is produced, the carbonic acid solidifies, and descends to about $-140°$. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out 3 litres per stroke, and the speed is 100 revolutions a minute. In the interior of the carbonic acid tube passes a fourth tube, 5 metres long, intended for the compression of oxygen. Being immersed in solid carbonic acid, the whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about 1 metre beyond the other. The small central tube screws into the neck of a large wrought-iron shell. This shell contains 700 grms. of chlorate of potash and 256 grms. of chloride of potassium mixed together, fused, then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the temperature to the required degree, the shell is heated over a series of gas-burners: the decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little and stops at 320 atmospheres. If at this moment the screw tap which terminates the tube is opened, a jet of liquid is distinctly seen to escape with extreme violence. If the tap is closed a second jet can be obtained in the course of a few moments. If pieces of charcoal, slightly incandescent, are put in this jet they inflame spontaneously with great violence. M. Pictet has already announced his intention of attempting the condensation of hydrogen and nitrogen by means of a similar apparatus to that we have described We shall hope to give the results of his further experiments in a future number of this Journal. Meanwhile we would refer our readers to the "Chemical News" of January 4 for a detailed description and diagrams of the complicated apparatus, by means of which M. Pictet has succeeded in liquefying oxygen.

Mr. Charles Stodder, in the "American Journal of Microscopy" (vol. ii., page 142), recommends the use of chloroform as a preparation for mounting dried Algæ in balsam. The immersion in chloroform has the effect of restoring the Algæ to their natural shape: the portions selected are then placed on a slide, and just before the chloroform evaporates the balsam is dropped on, and the object covered in the usual manner. The balsam should be allowed to harden slowly, as if heat is applied the specimen is liable to be shrivelled. The process is of especial value in mounting diatoms *in situ.*

The "American Journal of Microscopy" (vol. ii., p. 129) contains some remarks on the examination of objectives, by Ernest Gundlach, which may prove interesting to microscopists. The way in which various forms of spherical and chromatic aberration render themselves apparent are familiarly described. The subjects of angular aperture and working distance are discussed, and it is shown that considerable resolving power is only obtainable at the sacrifice of some portion of the latter quality of the objective. The immersion system is shown to be a substitute for the extreme thickness of the front lens needed in correction, formed by a corresponding stratum of water, to gain with the highest performance a tolerable working distance.

Our readers have probably noticed a recent addition to our fictile manufactures of a number of ornamental vases, cups, bowls, &c., of clear white glass, covered with beautiful iridescent films of different colours. At first it was thought that the process consisted in submitting the glass to the action of a deoxidating flame, and that the colours were caused by the reduction of the lead; from the specification of the patent, however, we find that the inventor of the process is M. L. Clémandot, a French civil engineer, who has patented it in France, England, and America, and that the principle of the process consists in submitting the glass vessels to the action of dilute hydrochloric, sulphuric, or other acid, under a pressure of from two to six atmospheres. M. Clémandot claims to be able to imitate the beautiful nacreous films on ancient glass which has been submitted to the combined action of air and water for two or three thousand years; but the ornamental vessels already exhibited, although very pretty, are a long way off the poorest specimens of Assyrian or Egyptian glass in any ordinary collection. Time is evidently an important factor in bringing about this singular change.

The injurious influence of the products of combustion of coal-gas upon the leather bindings of books is only too well known. Vellum seems unaffected; morocco suffers least; calf is much injured, and Russia still more so. The disintegration is most rapid with books on the upper shelves of a library, whither the heated products of combustion ascend, and where they are absorbed and condensed. By comparing specimens of old leather with specimens of new it is quite clear that the destructive influence of gas is due mainly to its sulphur. True there are traces of sulphates in the dye and size of new leather bindings, but the quantity is insignificant, and there is practically no free sulphuric acid. That leather may be destroyed by the oil of vitriol produced by the burning of gas in a library is proved by the following observations and analyses of Prof. A. H. Church, of the Royal Agricultural College, Cirencester. The librarian of one of our public libraries forwarded to him the backs of several volumes which had been "shed" by the books on the upper shelves in an apartment lighted by gas. The leather of one of these backs (a volume of the "Archæologia") was carefully scraped off so as to avoid removing any paper or size from beneath. This task of scraping was easy enough, for the leather was reduced to the consistency of Scotch snuff. On analysis of the watery extract of this leather the following figures were obtained:—

Free sulphuric acid in decayed leather	6·21 p.c.
Combined „ „ „	2·21 „
Total „ „ „	8·42 „

Writing of rare minerals found in Colorado, Mr. T. F. Van Wagenen, in the "Engineering and Mining Journal," says that thallium, indium, and cadmium have lately been detected in ores from that state. Of the rarer metals there have been found in Colorado, besides the three mentioned above, nickel, cobalt, selenium, tellurium, uranium, bismuth, molybdenum, and platinum, and there is scarcely a doubt that columbium, thorium, titanium, and vanadium will be recognised as soon as proper search is made. A belt of telluretted veins is believed to traverse the entire state from north to south. Two years ago sylvanite and alaite were found in San Juan county. The principal locality for bismuth ores is in Geneva, where two mines are being worked that carry a considerable quantity of schirmerite. Sulphide and carbonate of bismuth occur on Sugar-loaf Mountain, Boulder county. Nuggets of native bismuth are common in the upper gulches of the Blue Valley; the same metal has been found also in the Arkansas Valley. Nickel ore, ranging from 2 to 5 per cent, has been found in three localities. Among the mineralogical curiosities of the tellurium belt may be mentioned a telluride of mercury found in the Mountain Lion Mine. Native mercury and amalgams of both gold and silver have also been found at several points along this belt.

We are likely soon to hear of columbium-plating, a metal which is very much like nickel, but whiter, like tin. It was first found, more than fifty years ago, in the United States, near Middletown, Conn., and named in honour of America. It was thus far very scarce, but has now been found at Marion, N.C., and also in the Amazon stone of Colorado. If it becomes abundant enough to be used in the arts we may soon have a valuable addition to the useful metals, and also to their compounds or salts. The metal forms an acid like tin, called columbic acid; tin forms stannic acid, and the compounds of this acid with bases are stannates, very useful in the arts; the compounds of columbic acid with bases must be called columbates. They have not yet been investigated, but will likely be found extremely useful for some purposes.

As a means of preventing the explosion of fire-damp in coal mines M. Basin recommends ventilation with compressed air, watering the galleries (in dry mines), and the use of Bastie's toughened glass instead of wire-gauze for the cylinders of safety lamps. The tube which gives vent to the products of combustion will contain several superimposed layers of gauze.

From the "Mineral Statistics of Victoria for the Year 1876" we learn that the total yield of gold for that year in this colony has been 357,901 ozs. from alluvial sources, and 605,859 ozs. from quartz rock. The total yield of silver was 26,356 ozs., the greater portion of which was parted from gold smelted at the Mint. The exports of tin were 34 tons 9 cwts. During the year 2529 tons of antimony ore were raised, and 606 tons of ore, 474 tons of regulus, and 254 tons of crude antimony were exported.